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# "We the People of the United States": The Bicentennial of a People's Revolution

RICHARD B. MORRIS

"IT IS QUITE OBVIOUS," said Richard M. Nixon in March 1976, "that there are certain inherently governmental actions which, if undertaken by the sovereign in protection of the interest of the nation's security, are lawful but which, if undertaken by private persons are not." Nixon was responding in writing to questions put to him by the Senate Select Committee on Intelligence, concerning alleged abuses of the executive power during his administration.[1] That a former president of the United States should, during the course of the Bicentennial year, advance the notion that the executive is the sovereign seems not only ill-timed but curiously at variance with the principles for which the American Revolution was fought and upon which the Federal Constitution was framed. Gouverneur Morris, whose felicitous choice of the phrase "We the People of the United States" designated the people of the whole nation as the source of sovereignty, said of the president: "This Magistrate is not the King but the prime-Minister. The People are the King."[2] Morris was a better statesman than a prophet. He did not live to see the imperial presidency.

The Nixon commentary was of a piece with the attitude of the national government toward the commemoration of the Bicentennial. The two words most muted during the two hundredth year of American independence have been "people" and "revolution." But government officials are not the only ones who are guilty of a benign neglect on this score. A recent massive treatment of the War for Independence has as its subtitle *A People's History of the American Revolution*.[3] Therein one might expect to find the focus shifted from the elite to the common people, to the private in the ranks and the civilian on the home front. Alas, we have good old-fashioned narrative history, often written with compelling power, but withal a traditional military and political account. One would have to search elsewhere in sources and monographs comparatively discrete to understand why this war was indeed a "people's revolution," to divine how ordinary people gave a distinct cast to what was an extraordinary event for its own time, and to comprehend why, after two

This presidential address was delivered at the annual meeting of the American Historical Association in Washington, December 28, 1976.

[1] New York *Times*, March 12, 1976, 14:2.

[2] C. C. Tansill, comp., *Documents Illustrative of the Formation of the Union of the American States* (Washington, 1927), 421.

[3] Page Smith, *A New Age Begins: A People's History of the American Revolution* (2 vols., New York, 1976).



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centuries, the American Revolution can still inspire and admonish the American people.

To explain an epoch as complex as the American Revolution and to understand how a durable constitutional order evolved therefrom one must first recognize how so many aspects of the movement touched a popular chord. Mobilization of public opinion was central to the winning of a war for political independence which was at the same time the first successful effort in the modern era by a colonial people to sever an imperial connection with a great world power. Public sentiment clearly dictated the replacement of an unresponsive and corrupt monarchical system by a republic founded on public morality and, through the elective system and representative institutions, recognizing the sovereignty of the people.

If the American Revolution gave birth to a new nation, different from the nations of the Old World and destined to fix the direction of modern nationalism, its distinctive character was shaped in no small part by the enormous demographic changes in the two generations prior to the Revolution and by a popular culture which geographical separation, environmental factors, and sociocultural forces nurtured. It is a commonplace that out of the principal issue concerning the relation of colonies to metropolis the American Revolution forged an anticolonial principle, once central, later peripheral, but still troubling the American conscience. That principle was in obeisance to the sentiments of sovereign people on the move, giving reality to the paper boundaries drawn by the Peace of Paris in 1783. It was popular apprehension about the aggrandizement of power that prompted the promulgation of novel constitutional principles governing the relation of the state to the people, that recognized certain rights of the individual to be inherent, and that under the rubric of federalism imposed limitations on the authority of the central government. That other dimension of the American Revolution, a transforming egalitarianism, was manifest in the spirit of reform which characterized the American Revolutionary epoch and in the noteworthy degree of upward political mobility which was achieved.

The Founding Fathers never doubted the central role of the people in bringing about the final break with England. Time after time they felt impelled to pay obeisance to the people's participation from the start of hostilities in order to refute customary Loyalist and British accusations that the break with the mother country resulted from a deep-rooted conspiracy of the leadership.[4] That last point was so much "moonshine," George Mason charged. Writing from Gunston Hall in 1778 to an English correspondent, the Virginian asserted:

There never was an idler or a falser Notion than that which the British Ministry have imposed upon the Nation "that this great Revolution has been the Work of a Faction,

[4] Among others, John Jay, for example, felt impelled to deny, as late as December 1775, that the Continental Congress aimed at independence. "Proofs that the Colonies Do Not Aim at Independence" [Philadelphia, after December 11, 1775], in Richard B. Morris *et al.*, eds., *John Jay: The Making of a Revolutionary, Unpublished Papers, 1745–1780* (New York, 1975), 198–201.

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of a Junto of ambitious Men against the Sense of the People of America." On the Contrary, nothing has been done without the Approbation of the People, who have indeed out run their Leaders; so that no capital Measure hath been adopted, until they called loudly for it: to any one who knows Mankind, there needs no greater Proof than the cordial Manner in which they have co-operated, and the Patience and Perseverence with which they have strugled under their Sufferings; which have been greater than you, at a Distance, can conceive, or I describe.[5]

So constant and so consistent was the Revolutionary elite in acknowledging the people's indispensable role, in adopting the rhetoric of the transforming democratic radicalism of the American Revolution that, as one recent scholar sees it,[6] the leadership contributed to its own political demise. By the last quarter of the twentieth century the common man had replaced the uncommon man in the nation's leadership, a process that seems irreversible.

If the focus here is on the ordinary person of the American Revolution it is not from any intention to slight the role of the Patriot elite. Their concern with legality and constitutional principles, along with their sense of moderation, distinguished the American Revolution from almost all others to follow. Possessing a keen perception of the national interest, they made sure that a hoop encircled the barrel and bound the thirteen staves together. Their conviction that America had a unique role as a symbol of freedom gave the new nation the twin notions of mission and asylum—serving to render it distinctive among the states of the world.[7]

For almost two centuries now historians and editors have paid deference to the revolutionary leaders. Their virtues have been extolled, their faults exposed, their differences expatiated upon, their motives defended or impugned. Even their sex lives have been delved into and their psyches analyzed. Above all, their compulsive literary energies have inspired scholars to embark upon new, accurate, full, and erudite editions of their writings, a notable series of projects sponsored by the National Historical Publications and Records Commission.

And now it is the people's turn. The generic term "people" encompasses all the inhabitants of the thirteen colonies, not even excluding the upper ranks of society whose roles have been so well publicized. It includes whites and blacks, men and women, slave and free. It encompasses the stout-hearted Patriot and the fair-weather Patriot. It embraces the Loyalists, or "Petticoat Gentry," who, whether from conviction or expediency, made haste to tear up their wives' underthings and affix a red ribbon to their hats. It includes fence-sitters or neutralists, and that final group that confounds all quantifiers—those who took oaths of loyalty to both sides.

[5] George Mason to [Mr. —— Brent?], October 2, 1778, in Robert A. Rutland, ed., *The Papers of George Mason, 1725–1792* (Chapel Hill, 1970), 1:435.

[6] Gordon S. Wood, "The Democratization of Mind in the American Revolution," in *Leadership in the American Revolution*, Library of Congress Symposia on the American Revolution (Washington, 1974), 84.

[7] See my *Seven Who Shaped Our Destiny: The Founding Fathers as Revolutionaries* (New York, 1975); "The American Dream Among Nations—What Promise? What Fulfillment?" in *America at 200*, Foreign Policy Association, Headline Series, number 227 (New York, 1975), 3–35.

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For this paper "the people" of the Revolution is limited to the inhabitants who contributed to the decisions of revolution, change, and reform. This segment of the population comprises the free white males, mostly adults, who shared the Patriot viewpoint. As regards women, their part in maintaining morale was critical, and their vigilant mobilization against violators of nonimportation agreements, hoarders, and profiteers is a story by itself. Women were, however, excluded from the political process not only in America but everywhere, with New Jersey for a brief idiosyncratic period as a possible exception. Such exclusion accorded with the conventional view of male politicians. Thirty years after the start of the American Revolution, Secretary of the Treasury Albert Gallatin had the presumption to suggest to President Thomas Jefferson that the latter might consider women for public service and elicited this sharp rejoinder: "The appointment of a woman to office is an innovation for which the public is not prepared, nor am I."[8]

Second, Indians and blacks must also be excluded. Since the American revolutionaries fought for empire over the West as well as for their own freedom in the East, the Indian was regarded as being outside the constituency. Despite the theoretical commitment in the Declaration of Independence to the principle of equality and the antislavery convictions of certain northern Patriot leaders, blacks were usually not considered part of the constituency. They *were* not, and by the nature of their situation, *could* not be effectively recruited for the Patriot cause. Antislavery sentiment failed to prove a decisive factor in the contest for the loyalty of the Negro in the Revolution. Indeed it is doubtful that it could have overcome deep-seated resistance.[9]

Third, we must exclude the white bondsmen. They lacked that proof of independence normally regarded as a requisite to political activity. True, their status was temporary. But, excepting apprentices in the towns who participated in anti-British demonstrations,[10] the indentured servants were not mobilized for the Revolution. Save in cases of bound servants gaining their freedom by enlistment, often over their masters' vehement protests, the Revolution did nothing to end and little to ameliorate white bondage. What makes the point critical is that by the eve of the war, thanks to a rising wave of

[8] Thomas Jefferson to the Secretary of the Treasury, January 13, 1807, in Paul Leicester Ford, ed., *Writings of Thomas Jefferson* (New York, 1892–99), 9:7. See also Joan Hoff Wilson, "The Illusion of Change: Women and the American Revolution," in Alfred F. Young, ed., *The American Revolution: Explorations in the History of American Radicalism* (DeKalb, Ill., 1976), 383–445.

[9] The view of Winthrop D. Jordan, *White Over Black: American Attitudes Toward the Negro, 1550–1812* (Chapel Hill, 1968), 342, 244, that the success of antislavery in the last quarter of the eighteenth century was almost within reach is controverted by David Brion Davis, *The Problem of Slavery in the Age of Revolution, 1770–1820* (Ithaca, 1975), 255–57, and Edmund S. Morgan, "Slavery and Freedom: The American Paradox," *Journal of American History* 59 (1972):6. For the role of blacks in the Revolution, see Benjamin Quarles, *The Negro in the American Revolution* (Chapel Hill, 1961), 111–57; George H. Moore, "Historical Notes on the Employment of Negroes in the American Army of the Revolution," *Magazine of History, with Notes and Queries*, No. 1 (1907); Richard B. Morris, *The American Revolution Reconsidered* (New York, 1967), 72–76. For the unfulfilled expectations of some black Tory refugees, see G. Halliburton, "The Nova Scotia Settlers of 1792," *Sierra Leone Studies*, N. S., No. 9 (December 1957): 16–25; Anthony Kirk-Greene, "David George: The Nova Scotia Experience," *Sierra Leone Studies*, N. S., No. 14 (December 1960):93–120.

[10] Dirk Hoerder, "Boston Leaders and Boston Crowds, 1765–1776," in Young, ed., *The American Revolution*, 242, 248. For apprentices in the Philadelphia militia, see Eric Foner, *Tom Paine and Revolutionary America* (New York, 1976), 65, 126.

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redemptioner immigration, to a substantial traffic in white convict servants, and to an expanding roll of judgment debtors sold into servitude, the number of white servants held by indenture for limited terms exceeded the totals for any other period in colonial history.[11]

Even with these exclusions we still have an impressive number of persons of whom the leadership had to take account. Consensus historians prefer to see this mass as bound together by middle-class aspirations, if not necessarily of the middle class themselves. One perceptive historian speaks of "a numerous, relatively aggressive, and largely enfranchised middle-class public."[12] True enough, America, lacking a titled aristocracy, offering a widely distributed freehold tenure, and occupied by a large English-speaking Protestant population, presented an image of homogeneity and classlessness. It is now recognized, however, that the assumption that America skipped the feudal stage of history, like other oversimplifications, conveys a partial truth, while ignoring or minimizing not only the remnants of feudalism still present in pre-Revolutionary America but even, as some historians now insist, an increasing feudalization or Europeanization of the American scene, a process productive of social tensions.[13]

Everywhere one finds inequality. Everywhere colonial society was divided into ranks or orders. Though affluence increased in the eighteenth century, so did poverty. While the standard of living of craftsmen, small shopkeepers, and laborers improved, their position vis-à-vis the more prosperous merchants deteriorated. In selected rural areas the same disparity was found.[14] The poorest taxpayers in the two generations preceding the Revolution enjoyed a continued rise in income while suffering a declining share of the assessed taxable wealth.[15] Retrospectively such disparities in income between the top

[11] See Richard B. Morris, *Government and Labor in Early America* (New York, 1946), 147–49, 314 n., 326, 362–63. The indentured servants were increasingly concentrated in rural areas, while towns like Boston were suffering a relatively rapid decline in immigration. James A. Henretta, "Economic Development and Social Structure in Colonial Boston," *WMQ*, 3d ser., 22 (1965):83. In Philadelphia and its environs, the importation of German and Scotch-Irish redemptioners recommenced at the end of the Seven Years' War, while slave imports declined. Gary B. Nash, "Slaves and Slaveowners in Colonial Philadelphia," *WMQ*, 3d ser., 30 (1973), 223–56. Nevertheless, it is estimated that slaves and servants together of working age declined from 21 percent of Philadelphia's population (1767) to 16 percent (1775) and 5.5 percent (1783).

[12] Richard Hofstadter, *America at 1750: A Social Portrait* (New York, 1971), 131–33.

[13] Richard B. Morris, *The American Revolution Reconsidered* (New York, 1965), 60–65; Rowland Berthoff and John M. Murrin, "Feudalism, Communalism, and the Yeoman Freeholder: The American Revolution Considered as a Social Accident," in Stephen G. Kurtz and James H. Hutson, eds., *Essays on the American Revolution* (Chapel Hill, 1973), 256–88; Jack P. Greene, "The Social Origins of the American Revolution: An Evaluation and Interpretation," *PSQ*, 88 (1973):1–22; Kenneth A. Lockridge, "Social Change and the Meaning of the American Revolution," *Journal of Social History*, 6 (1972–73):403–59. For the argument that, so far as New York was concerned, the neofeudal trappings of the great manors had little meaning by the mid-eighteenth century, see Sung Bok Kim, "A New Look at the Great Landlords of Eighteenth-Century New York," *WMQ*, 3d ser., 27 (1970):581–614; Patricia U. Bonomi, *A Factious People: Politics and Society in Colonial New York* (New York, 1971), 196–200. For a re-evaluation of the impact of quitrents in one royal colony, see Alan D. Watson, "The Quitrent System in Royal South Carolina," *WMQ*, 3d ser., 33 (1976):183–211.

[14] See Henretta, "Economic Development and Social Structure," 75–92; Allan Kulikoff, "Progress of Inequality in Revolutionary Boston," *WMQ*, 3d ser., 28 (1971):375–412.

[15] See James T. Lemon and Gary B. Nash, "The Distribution of Wealth in Eighteenth-Century America: A Century of Change in Chester County, Pennsylvania, 1693–1782," *Journal of Social History*, 2, No. 1 (1968):1–24. For the view that the small man's economic position advanced in eighteenth-century Virginia, see Edmund S. Morgan, *American Slavery–American Freedom: The Ordeal of Colonial Virginia* (New York, 1975), 338–46.

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and bottom ranks seem the inevitable result of the vast expansion of commercial farming and large-scale commerce. Even in New England, land of subsistence farming, population growth significantly decreased the land available per person and contributed to economic polarization.[16] To compound the antagonism between ranks in society, an amorphous body of free-floating workers, unwelcome in many cases, sharing a common poverty, now posed an increasing problem to an urban society which was slowly assuming some of the characteristics of modernization, with its spreading commercialism, occupational specialization, and social atomization.[17]

In essence, whether we are considering the assumptions of present-day historians, divided on the significance of urban poverty in the pre-Revolutionary period, or examining the testimony of contemporaries, the prospect depends on the eye of the beholder. Where Crèvecoeur saw the American dwelling in a climate of "pleasing equality,"[18] John Day, a Nova Scotia merchant who spent a good deal of time in the other North Atlantic port cities, pictured a "rising aristocracy" and a "numerous vagrant poor."[19]

Admittedly, the American Revolution did not witness an uprising of the *sans-culottes* like that of France some years later. It is hard to make out of it a class struggle, when the very term "class" did not yet enjoy wide currency.[20] Notwithstanding, the attachment to the cause of revolution by the lower and middling orders, as they were then known, was central both to its initiation and its ultimate success. These orders were distinguished from their "betters" by dress, and often, too, by speech, manners, and habits. *M'Fingal* might jeer at "each leather-aproned dunce grown wise," a point of view shared by many who wore silks and velvets, covered their heads with powdered wigs, wore silver-buckled shoes, and rode in chaises. What surprised them more often than not was how the "village Hampden" and the "mute inglorious Milton" made their discontent known, though deemed to be inarticulate. Their use of songs, jigs, and toasts, of effigies, parades, and demonstrations, even of mobbing and tarring and feathering proved that the inarticulate were by no means mute.[21] Neither literary correspondents nor diarists, careless in preserving farm and account books, they wrote the stories of their lives in baptismal registers and on gravestones, in the court records, the deeds and wills, the inventories of estates, the assessment, tax, and tithable rolls, the militia lists, and in countless newspaper notices placed by craftsmen, shopkeepers, and owners of runaway servants.

[16] Kenneth A. Lockridge, "Land, Population, and the Evolution of New England Society, 1630–1790," *Past and Present*, 39 (1968):62–80.

[17] Gary B. Nash, "Poverty and Poor Relief in Pre-Revolutionary Philadelphia," *WMQ*, 3d ser., 33 (1976):3–30.

[18] Michael Guillaume Jean de Crèvecoeur, *Letters from An American Farmer* (London, 1782), Letter III.

[19] Jack P. Greene, "Social Structure and Political Behavior in Revolutionary America: John Day's Remarks on American Affairs (London, May 7, 1774)," *WMQ*, 3d ser., 32 (1975):481–94.

[20] The *Oxford English Dictionary* lists a use of "class" in the sense of "orders" of society as early as 1656, but it was not in common currency for well over a century. See also Raymond Williams, *Culture and Society, 1780–1950* (New York, 1960), xii.

[21] See Jesse Lemisch and John K. Alexander, "The White Oaks, Jack Tar, and the Concept of the 'Inarticulate'," *WMQ*, 3d ser., 29 (1972):109–34.

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United only in resenting privilege, the lower ranks of society voiced different grievances in different areas. In interior Massachusetts the court system and the aggrandizement of power by the justices of the peace aroused their ire.[22] In Vermont, New York, and the Jerseys, New York patentees, manor lords, or the Jersey proprietors blocked the settlers' quest for fee-simple lands.[23] Pennsylvania found frontiersmen arrayed against the Eastern inhabitants, while the back country regulators of the Carolinas registered protests against regressive taxation, unequal representation, inequitable or inadequate justice,[24] and in the southern towns white mechanics joined to limit the employment of slaves in the handicraft trades.[25]

The severe social strains which so many members of the lower orders experienced, while perhaps accounting for some of the frenzied rhetoric of the period,[26] failed to unite all the aggrieved, if disparate, groups in support of independence. Contrariwise, where landlords happened to be stout Whigs, tenants understandably became Tories. Imperial measures aroused the seaboard from end to end, while the frontier remained sharply divided in allegiance. It is not a fictitious unity of the aggrieved inhabitants that is relevant to the Revolution, but rather the evidence that the various movements they began, to eliminate inequities in society, gathered strength and thereby defined the reform goals of the Revolution.

If a large segment of the lower orders in rural areas defected from the Patriot cause, the reverse was true of towns wherein the action was centered. With few exceptions, the town mechanics, laborers, and seamen were either involved in the pre-Revolutionary agitation or were swept up into the Revolution. When we use the word "mechanics" we are employing a catchall covering anyone who worked with his hands, including master artisans and journeymen wage-earners (tomorrow's master mechanics) and, even more broadly, comprehending all groups below the ranks of merchants and lawyers. Ranging from skilled and creative silversmiths and cabinetmakers to common laborers and cartmen, from master craftsmen who owned their shops and employed journeymen and apprentices to the unskilled dock workers, the "mechanics," with their families, made up a respectable segment of the inhabitants of the major Revolutionary ports and of the smaller towns.[27] Like

[22] John M. Murrin, "The Legal Transformation," in Stanley N. Katz, ed., *Colonial America: Essays in Politics and Social Development* (Boston, 1971), 415–49.

[23] See Edward Countryman, "Out of the Bounds of the Law: Northern Land Riots in the Eighteenth Century," in Young, ed., *The American Revolution*, 39–69; Marvin L. Michael Kay, "The North Carolina Regulation, 1766–1776: A Class Conflict," *ibid.*, 73–123.

[24] See, e.g., Kay, *ibid.*; Robert A. Becker, "Revolution and Reform: An Interpretation of Southern Taxation, 1763 to 1783," *WMQ*, 3d ser., 32 (1975):417–42.

[25] Morris, *Government and Labor*, 184–86.

[26] This is an argument in Gordon S. Wood, "Rhetoric and Reality in the American Revolution," *WMQ*, 3d ser., 23 (1966):3–32. The extent to which religious revivalism in the colonial interior infected the rhetoric of the time is suggestively treated by Rhys Isaac, "Preachers and Patriots: Popular Culture in Revolutionary Virginia," in Young, ed., *The American Revolution*, 125–54.

[27] See Staughton Lynd and Alfred F. Young, "After Carl Becker: The Mechanics in New York City Politics, 1774–1801," *Labor History*, 5 (1964):215–24. The Philadelphia mechanics were mainly independent entrepreneurs, according to Charles S. Olton, *Artisans for Independence: Philadelphia Mechanics and the American Revolution* (Syracuse, 1975), 38. In Philadelphia the tools alone which master mechanics possessed seemed to

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the Parisian *sans-culottes*, they were "the people . . . without the frosting."[28] Masters or journeymen mechanics, like so many other people of the lower and middling orders, they did not exclusively identify themselves with a single category or regard their status as permanent, any more than the American worker does at the present time.[29]

In the towns the Revolutionary movement drew its strength from the "crowd" or the "mob," depending on whether one views the political actions as purposeful and disciplined or lawless and subject to manipulation, with most modern and contemporary scholars taking the former view.[30] If not manipulated, the crowd clearly shared the political principles of its leaders who endorsed the contemporary radical Whig ideology, stressing constitutional rights and political liberty. Beyond that one detects among the lower orders an oft-voiced bias against wealth and privilege,[31] along with something of that "moral economy" which a current student of mass behavior finds to have informed crowd action in England.[32] On this side of the ocean, food riots and demonstrations against engrossers and price-fixing violators attest to the transplantation of some of these values.[33] Perhaps the most sensational incident occurred in Philadelphia well on in the war, when an inflamed populace attacked "Fort Wilson," James Wilson's residence and refuge for a group of his republican merchant friends commonly believed to have manipulated the price of goods.[34]

It may be straining the evidence to establish a genealogical nexus between components of crowds that rioted against smallpox inoculations, protested grain shortages, and tore down brothels. On the other hand, the impressment riots mobilized seamen, dock-workers, and the entire maritime industry

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constitute sufficient property for most to qualify for the suffrage. *Ibid.*, 51, 52. See also David Montgomery, "The Working Class of the Pre-Industrial City, 1780–1830," *Labor History*, 9 (1968):5; Roger J. Champagne, "Liberty Boys and Mechanics of New York City, 1764–1774," *Labor History*, 8 (1967):124, 125. For estimates of the working-class population, see Carl Bridenbaugh, *Cities in Revolt: Urban Life in America, 1743–1776* (New York, 1955), 283; Benjamin W. Labaree, *Patriots and Partisans: The Merchants of Newburyport, 1764–1815* (Cambridge, Mass., 1962), 4–5. For the enlistment of master ship carpenters in the nonimportation movement, see James H. Hutson, "An Investigation of the Inarticulate: Philadelphia's 'White Oaks'," *WMQ*, 3d ser., 28 (1971), 3–25.

[28] R. R. Palmer, "Popular Democracy in the French Revolution: Review Article," *French Historical Studies*, 1 (1960):453.

[29] For a comment on Lyndon B. Johnson's characteristic assumption of pluralist thought as regards class or occupation, see Doris Kearns, *Lyndon Johnson and the American Dream* (New York, 1976), 156, 157.

[30] For the former view of English and continental crowd action, see George Rudé, *The Crowd in History, 1730–1848* (New York, 1964), 52 ff. A similar view of the American revolutionary crowd is found in Bernard Bailyn, ed., *Pamphlets of the American Revolution, 1750–1776* (Cambridge, Mass., 1965– ), 1:581–83, 740 n., and in Pauline Maier, *From Resistance to Revolution: Colonial Radicals and the Development of American Opposition to Britain, 1765–1776* (New York, 1974). A dissenting view of Hiller B. Zobel, *The Boston Massacre* (New York, 1970) has been vigorously criticized by Jesse Lemisch, review article in *Harvard Law Review*, 84 (1970):485–504, and by Pauline Maier, *The Journal of Interdisciplinary History*, 2 (1971):119–35.

[31] Dirk Hoerder, "Boston Leaders and Boston Crowds, 1765–1776," in Young, ed., *American Revolution*, 235–71.

[32] E. P. Thompson, "The Moral Economy of the English Crowd in the Eighteenth Century," *Past and Present*, 50 (1971):76–136.

[33] For the grass-roots support for price-fixing in the Revolution, see Morris, *Government and Labor*, 127–32. See also Ronald Hoffman, "The 'Disaffected' in the Revolutionary South," in Young, ed., *American Revolution*, 282, 298.

[34] See, e.g., John K. Alexander, "The Fort Wilson Incident: A Case Study of the Revolutionary Crowd," *WMQ*, 3d ser., 31 (1974):589–612.

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against the Royal Navy and helped cement the alliance between maritime workers and merchant shippers against customs and naval authorities, an alliance so fateful in its consequences.[35] In other trades as well, labor's grievances against employers over work and wages were submerged as both sides found common cause in resisting British policies.[36]

Remote as historians of today are from the violent temper of pre-Revolutionary times, it is indubitably easier for us to view these mounting demonstrations with a detachment understandably lacking among the victims of mobbism. Henry Laurens suffered a visitation by a crowd during the Stamp Act furor and castigated the Sons of Liberty "Devil Burners" of Charleston as a "Mob" which had "committed unbounded acts of Licentiousness and at length Burglary and Robbery."[37] Some time later, when he had become a true-blue Patriot, Laurens was accused by Daniel Moore, the customs collector, of raising "a Mob" against him. "No such thing as Mobbing was ever thought of," Laurens protested. The crowd that surrounded the king's officer did nothing worse than threaten to cut off his ears![38]

The participation of the mechanics had a profound effect upon the commitment of the leading towns toward the Revolution and helped radicalize the movement of imperial protest. Henceforth leaders like John Jay, who were conciliatory on principle but activist by temperament, recognized the signals and joined the revolutionary current.[39]

The crowd action that propelled the American towns along the road to independence has been examined so often that the sequence of events in which the crowd participated against British measures hardly needs an accounting.[40] From the Stamp Act riots to the Boston Tea Party, a logical progression of crowd demonstrations responded to British programs and policies. Behind crowd action one uncovers an elaborate Patriot infrastructure facilitated by the unities of everyday life in preindustrial American towns.[41] Boston, for example, found radical operations shared by the Loyal Nine, both genesis and executive committee of the Sons of Liberty, the three area Caucus Clubs, and

[35] See Jesse Lemisch, "The Radicalism of the Inarticulate: Merchant Seamen in the Politics of America," in Alfred F. Young, ed., *Dissent: Explorations in the History of American Radicalism* (DeKalb, Ill., 1968), 39–82. Richard M. Brown, "Violence in the American Revolution," in Kurtz and Hutson, eds., *Essays on the American Revolution*, 66, has identified seventeen Boston riots out of thirty anti-British demonstrations that were directed against customs enforcement and six over impressment by the Royal Navy.

[36] Morris, *Government and Labor*, 188–93.

[37] Henry Laurens to Joseph Brown, October 22, 1765, in George C. Rogers, Jr., and David R. Chesnutt, eds., *The Papers of Henry Laurens*, 5 vols. (Columbia, S. C., 1968– ), 5:27; Laurens to James Grant, November 1, 1765, *ibid.*, 39, 40.

[38] Laurens to Ross and Mill, October 8, 1767, *ibid.*, 339–41.

[39] See Letter of the Committee of Sixty to the New Haven Committee, April 17, 1775, in Morris, ed., *John Jay*, 143–44.

[40] The Sons of Liberty and Sons of Neptune, which prepared the early organization and mobilization of the crowd, have been studied by, among others, Henry B. Dawson, *The Sons of Liberty in New York* (New York, 1859); Herbert M. Morais, "The Sons of Liberty in New York," in Richard B. Morris, ed., *The Era of the American Revolution* (New York, 1939); Roger J. Champagne, "New York Radicals and the Coming of Independence," *Journal of American History*, 51 (1964):21–40; Richard Walsh, *Charleston's Sons of Liberty: A Study of the Artisans, 1763–1789* (Columbia, S. C., 1959).

[41] See Sam Bass Warner, Jr., *The Private City: Philadelphia in Three Periods of Its Growth* (Philadelphia, 1968), 24.

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assemblages at favorite meeting places—taverns, a distillery, and a newspaper office. The radical network linked artisans and mechanics, the church, the Boston town meeting, and the provincial council and assembly. At the center functioned the ubiquitous Samuel Adams, and around him the crowd leaders like Ebenezer Mackintosh.

Outside the towns, where the sanctions of the crowd did not reach, the militia provided a lever to move public opinion. With all its deficiencies, the militia posed to each able-bodied male a commitment of loyalty, thereby helping to politicize communities as well as individuals. As one historian has remarked, "firearms were great levelers, and the use of them by ordinary men against established authority was in itself enough to generate leveling thoughts."[42]

Together, popular assemblages and the militia recruited a broad-based support for revolution. Without such support the leadership never could have carried the day for independence, even allowing for the French alliance and British incompetence. In fact, demands for independence rose from the grass roots, and leaders, even Samuel Adams, reflected popular sentiment quite as much as they encouraged it.[43] On May 1, 1776, Joseph Hawley could write Elbridge Gerry: "For God's sake, let there be a full revolution, or all has been done in vain. Independence and a well planned continental government will save us."[44] When, nine days later, the Massachusetts House of Representatives put the question of independence to the towns,[45] the response of the inhabitants was emphatic. So mighty was the torrent of instructions and resolutions that poured forth from the towns and so various were the reasons vouchsafed for independence, that it would be straining the evidence to attribute these opinions to elitist leaders, however much the initiative had remained with the Boston Committee of Correspondence.[46] "Every Post and every Day rolls in upon us," said John Adams. "Independence like a Torrent."[47] Since popular-based support was not confined to Massachusetts, elsewhere decisions were made by large bodies of inhabitants assembled in town, county, or provincial meetings and responsive to public opinion. The reverse was of course true in Britain.[48]

[42] Morgan, *American Slavery–American Freedom*, 379; John Shy, *A People Numerous and Armed* (New York, 1976), 218, 220; Daniel J. Boorstin, *The Americans: The Colonial Experience* (New York, 1958), 345, 372.

[43] Pauline Maier, "Coming to Terms with Samuel Adams," *AHR*, 81 (1976):12–37.

[44] Joseph Hawley to Elbridge Gerry, May 1, 1776, in James T. Austin, *Life of Elbridge Gerry* (Boston, 1828–1829), 1:175–76. For the role of the Committee of Privates, a group of extreme radicals in the Pennsylvania militia, see Olton, *Artisans for Independence*, 74.

[45] Peter Force, ed., *American Archives: Fourth Series, Containing a Documentary History of the English Colonies in North America, from the King's Message to Parliament, of March 7, 1774, to the Declaration of Independence by the United States* (6 vols., Washington, 1837–46), 6:420.

[46] See, e. g., Instructions of Malden, May 27; Scituate, June 4; Resolve of Natick, June 20, 1776, Massachusetts Archives, lib. 156:fols. 101, 103, 113. See also Richard D. Brown, *Revolutionary Politics in Massachusetts: The Boston Committee of Correspondence and the Towns, 1772–1774* (Cambridge, Mass., 1970), 95–99, 208.

[47] John Adams to James Warren, May 20, 1776, "Warren-Adams Letters," Massachusetts Historical Society, *Collections*, 72 (1917):249–51.

[48] For the popular response in Philadelphia, see John J. Zimmerman, "Charles Thomson, 'The Sam Adams of Philadelphia'," *Mississippi Valley Historical Review*, 45, No. 3 (1958):464–80; for Charleston, see Walsh, *Charleston's Sons of Liberty*. See Ian R. Christie and Benjamin W. Labaree, *Empire or Independence, 1770–1776: A British-American Dialogue on the Coming of the American Revolution* (New York, 1976).

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How town meetings and county conventions were transformed into mass meetings of the "Body of the People" is the story of the way in which royal government was supplanted by extralegal bodies representing "the people," "the community," or "a majority of the community."[49] That story has a twofold significance. First, its telling is essential to an understanding of the formation of the Union; and second, the central role of the people transformed the American Revolution from a war for independence into a broad-based movement of change and reform.

As for the constitutional aspect of popular sovereignty, the record shows that selection of delegates to the First and Second Continental Congresses occurred largely outside the colonial legislative bodies. Selection was accomplished in different ways—by revolutionary committees, by the polling of freeholders, or by elections by illegal assemblies and revolutionary conventions. The use of conventions, so important in mobilizing public opinion not only during the war but in the years preceding Shays' Rebellion,[50] may have been modeled upon English usage in the seventeenth century, and the American example was closely followed by British reformers like James Burgh and the followers of Wyvill.[51]

In part, these extralegal methods of registering the public's will may have been fortuitous; in part, they sprang from the necessities of the revolutionary situation. The selection of delegates to the First Congress took place in the spring and summer of 1774 after the adjournment of the regular winter meeting of most colonial assemblies. Unless the colonial governors summoned special legislative sessions, the assemblies could not convene or act. To fill this vacuum of power, popular or revolutionary bodies chose a majority of the delegations to Congress. In fact, only four out of the twelve colonies represented in the First Continental Congress used their regular assemblies to elect delegates, and two of these bodies were extralegal.

Elsewhere I have detailed the steps by which the First and Second Continental Congresses were constituted and their respective relations to the colonies and the states.[52] Because the methods used to select delegates to both Congresses are crucial to our understanding of the forging of the union as well as popular sovereignty, I shall make these points here very briefly. For picking delegates to the First Congress, the speaker rather than the governor called the assembly into session in Delaware; in Massachusetts Bay, the General Court held its session, openly defying the governor. Only in Pennsylvania and Rhode Island did regular assemblies elect delegates to the Congress without interference from their respective governors, and both were exceptional colo-

[49] G. B. Warden, *Boston, 1689–1776* (Boston, 1970), 218–19; Brown, "Violence in the American Revolution," in Kurtz and Hutson, eds., *Essays on the American Revolution*, 108 ff.; Thad W. Tate, "The Social Contract in America, 1774–1787: Revolutionary Theory as a Conservative Instrument," *WMQ*, 3d ser., 22 (1965):375–91.

[50] Morris, *Government and Labor*, 117–18; Richard B. Morris, "Insurrection in Massachusetts," in Daniel Aaron, ed., *America in Crisis: Fourteen Crucial Episodes in American History* (New York, 1952), 28, 29.

[51] Arthur Sheps, "The American Revolution and the Transformation of English Republicanism," *Historical Reflections*, 2 (Summer 1975):16.

[52] Richard B. Morris, "The Forging of the Union Reconsidered: A Historical Refutation of State Sovereignty Over Seabeds," *Columbia Law Review*, 74 (1974):Appendix at 1091–93.

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nies. One was proprietary, the other self-governing—and in neither did the royal government exercise control over the convening of the legislature. With scrupulous attention to a choice of words, in formal resolutions after assembling, delegates to the First Congress described themselves as being appointed or elected "by the *good people* of the several colonies," and their secretary, Philadelphia's popular Whig leader Charles Thomson, underscored "good people."[53] The evidence demonstrating that selection of delegates to the First Congress occurred largely outside the colonial legislative bodies supports Joseph Story's contention that the Continental Congress was organized "with the consent of the people acting directly in their primary sovereign capacity," and reminds us that there was something more than the ritualistic rhetoric of the nationalists to his claim that the union was "spontaneously formed by the people of the United Colonies."[54]

Confident that its actions would find strong popular support and acting with less divisiveness than has traditionally been attributed to it,[55] the First Congress, as one of its concluding actions in October 1774, called for a successor Congress to convene on May 10, 1775. The instructions went out to all "the Colonies, in North-America" to choose deputies, with a separate appeal "to the Inhabitants of the Province of Quebec," urging them "to unite with us in the social compact." They were instructed to elect, at town and district meetings, deputies to a provincial congress, which in turn would choose delegates to the Congress at Philadelphia.[56] The procedure was extralegal, if not subversive, and involved a series of popular acts in open defiance of authority. This time around the delegates from the thirteen colonies were chosen during the winter and early spring when most provincial assemblies were holding their regular sessions. Anticipating that the crown-appointed governors would prorogue the assemblies rather than permit them to partake in the election process, the people in eight of the colonies chose their delegates through extralegal assemblies or conventions.[57]

As the authority of royal governors and colonial assemblies disintegrated and the upstart revolutionary congresses or conventions pointedly ignored both branches,[58] the Second Congress filled the continuing power vacuum by assuming the initiative in establishing revolutionary governments in the colonies and ultimately transforming them into states. Meanwhile, extralegal committees of public safety, patterned after the revolutionary committees in England during the Puritan Revolution and in the American colonies during the upheavals of 1689, assumed the direction of the revolutionary movement in

[53] Worthington C. Ford *et al.*, eds., *Journals of the Continental Congress, 1774–1789* (34 vols., Washington, 1904–37), 1:66 (October 14, 1774).

[54] Joseph Story, *Commentaries on the Constitution of the United States* (3d ed., Boston, 1858), 1:138, 142.

[55] See David Ammerman, *In the Common Cause: American Response to the Coercive Acts of 1774* (New York, 1976).

[56] Ford, ed., *Journals of the Continental Congress*, 1:102 (October 22, 1774); 105, 112 (October 26, 1774).

[57] For a criticism of the rump character of some of the extralegal elections, see "The Censor," an unidentified Philadelphia newspaper, March 5, 1776, in Force, ed., *American Archives: Fourth Series*, 5:71–72.

[58] For an informed review of this transitional period of governmental authority at the colony level, see Merrill Jensen, *The Founding of a Nation: A History of the American Revolution, 1763–1776* (New York, 1968), 508–34.

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several colonies. On July 18, 1775, the Continental Congress recommended that those colonies lacking such committees set them up to direct their defenses while the assemblies or conventions were in recess.[59] In some five colonies, committees of safety were established on Congress's initiative, and by 1777 every state had one.[60]

Not only did Congress turn to the people to set up a continental-wide revolutionary machinery, but it also took the lead in issuing a call to the people of the colonies to organize state governments, first on an *ad hoc* basis,[61] and, at length and in response to public opinion, it made this practice formal. On May 15, 1776, Congress issued its justifiably renowned resolution urging assemblies and conventions, "where no Government sufficient to the Exigencies of their affairs hath been hitherto established, to adopt such Government as shall, in the Opinion of the Representatives of the People, best conduce to the happiness and Safety of their Constituents in particular, and America in General."[62]

The twin processes—the election of delegates to the two Congresses and the procedures for framing state constitutions under Congressional directive—set loose a torrent of assertions that the people were the constituent power.[63] Some urged direct election of the delegates to Congress by "the freeholders, or freemen at large."[64] Others, like the "Mechanicks of New York City," insisted that the decision "to accept or reject a Constitution" was the "birthright of every man" who "is, or ought to be, a co-legislator with all the other members of that community."[65] To explain to a French visitor the convention and ratification procedures which produced the Massachusetts Constitution of 1780, Samuel Adams, voicing sentiments found in a declaration of the Massachusetts General Court in 1776 that "power resides always in the body of the people," pointed out that far more than the necessary two-thirds vote endorsed the Constitution, thus proving that "the true *sovereign* are the people."[66]

In adopting the Declaration of Independence, an act of paramount, sovereign authority, Congress acted for the people rather than for thirteen separate states, since only four state governments, three of them provisional, had been

[59] Ford, ed., *Journals of the Continental Congress*, 2:187–90.

[60] *Ibid.* See also Agnes Hunt, *The Provincial Committees of Safety of the American Revolution* (Cleveland, 1904), 159–65.

[61] For the authorizations to New Hampshire and South Carolina, see Jere R. Daniell, *Experiment in Republicanism: New Hampshire Politics and the American Revolution, 1741–1794* (Cambridge, Mass., 1970), 106–12; Edward McCrady, *The History of South Carolina in the Revolution, 1775–1780* (New York, 1901), 110, 115, 235.

[62] Ford, ed., *Journals of the Continental Congress*, 4:342. The resolve was voted on May 10; the preamble, written by John Adams, was adopted five days later, *ibid.*, at 357–58. See *Diary and Autobiography of John Adams*, Lyman H. Butterfield *et al.*, eds. (Cambridge, Mass., 1961), 3:335n., 382–86.

[63] See especially R. R. Palmer, *The Age of the Democratic Revolution, A Political History of Europe and America, 1760–1800: The Challenge* (Princeton, 1959), pp. 213–28.

[64] Force, ed., *American Archives: Fifth Series, Containing a Documentary History of the United States of America from the Declaration of Independence, July 4, 1776, to the Definitive Treaty of Peace with Great Britain, September 3, 1783* (3 vols., Washington, 1848–53), 2:113–14 (September 1776).

[65] *Ibid.*, *Fourth Series*, 6:895 (June 14, 1776).

[66] François Jean, Marquis de Chastellux, *Travels in North America, in the Years 1780, 1781, and 1782*, J. Kent, trans. (London, 1787), 1:268 ff. See also Oscar and Mary Handlin, eds., *The Popular Sources of Political Authority: Documents on the Massachusetts Convention of 1780* (Cambridge, Mass., 1966), esp. 51–53.

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formed prior to its passage. Jefferson's felicitous phrasing described "one people" as dissolving the political bands connecting them with another, affirmed that governments derived "their just powers from the consent of the governed," and proclaimed "the right of the people" to alter or abolish "a government destructive of the ends set forth." Good Whig rhetoric or political reality? The United States Supreme Court thought it was the latter. Members of the first Court, who—one might say—were present at the creation, characterized the Great Declaration as the act of the "whole people," one which transferred sovereignty "from the crown of Great Britain" to "the people." Chief Justice Jay, who never went back to Philadelphia to sign the document that New York's delegates had been originally enjoined from endorsing, might, if pressed, have conceded that the "whole people" excluded the substantial segment opposed to independence.[67] Then as now, one might add, the High Court demonstrated at times an uninhibited facility to generalize about complex issues in constitutional history. In the period of congressional government, running from September 1774 to March 1, 1781, prior to the Articles of Confederation, the Supreme Court perceived Congress as exercising powers derived from the people, expressly conferred through the medium of state conventions or legislatures, and, once exercised, "impliedly ratified by the acquiescence and obedience of the people."[68]

In sum, a review of the historical evidence makes abundantly clear that the Supreme Court accepted the rhetoric while recognizing the reality of the process whereby the United States was created by the people in collectivity, rather than by the individual states. The process of selecting the delegates for the First and Second Congresses underscores the revolutionary role of the people of the colonies in establishing a central governing body. Congress alone possessed those attributes of external sovereignty which entitled it to be called a state in the international sense, while the separate states, exercising a limited or internal sovereignty, may rightly be considered a creation of the Continental Congress, which preceded them and brought them into being.[69]

Without the attachment of a substantial body of the people to the cause of independence, that goal could not have been achieved, nor could the union have been forged, if it had not rested upon a national consensus.[70] Indubitably, the elite leadership recognized the urgency of establishing a national character. John Jay did more than coin a phrase when he observed in 1797: "I wish to see our people more Americanized, if I may use that expression; until we feel and act as an independent nation, we shall always suffer from foreign intrigue."[71] If emerging nationalism depended primarily

[67] *Chisholm* v. *Georgia*, 2 U. S. (2 Dall.) 419, 470–71 (1793).

[68] *Ware* v. *Hylton*, 3 U. S. (3 Dall.) 199, 232 (1796), (per Chase, J.). See also *Penhallow* v. *Doane*, 3 U. S. (3 Dall.) 54, 80, 90–94, 109–12, 117 (1795), (per Paterson, J.).

[69] Morris, "The Forging of the Union Reconsidered," 1088–89.

[70] This point is argued by Edmund S. Morgan, "The American Revolution: Was There 'A People'?," *The New York Review of Books*, July 15, 1976.

[71] John Jay to Colonel John Trumbull, Albany, October 27, 1797, (ALS advertised in American Book Prices Catalogue, 1897), in Henry P. Johnston, ed., *The Correspondence and Public Papers of John Jay* (New York, 1893), 4:232.

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on a political consensus,[72] then the boycotts, protest movements, and the élan generated in fighting for independence heralded the emergence of a distinctive American culture.[73] From the first, Dr. David Ramsay understood the springs of young America's cultural nationalism.[74] This sense of national purpose and uniqueness moved the common man, albeit with misgivings, to support a Constitution, whose ratification under a broad suffrage was so joyfully greeted by working-class people in every city in the land. With the aspirations of the common man in mind, we may see the Constitution, which assured the survival of a federal union, not as a counter-revolutionary event, but as part of the revolutionary process which had enlisted the energies and loyalties of masses of plain men and women for more than two decades.[75]

In the face of an impressive body of evidence of social tension and of mounting pressures to end inequities between ranks and regions, consensus historians still deny that the American Revolution was a rising of the masses against their oppressors.[76] Nobody today seriously argues that an internal war over who should rule at home was fought between the propertyless masses and the privileged minority. Property was too widely distributed or the expectation of possessing it too broadly held for society to divide over this issue. In fact, despite their many differences, the people did unite in a common cause. What unified the discordant elements of the Patriot populace was the conviction that only through independence could they build a free society. What informed their common purpose was a republican ideology that recognized the people as the constituent power. To fill the vacuum of governmental authority which the act of revolution created, it was necessary to build a new political structure both for the thirteen states and for the nation. In the ensuing series of experiments in constitution-making and legislation, a people, liberated from the strictures of the old colonial order, began an era of innovation unprecedented in that day and age.[77]

The constitutions of the states embodied safeguards for civil liberties, checks upon executive usurpation, frequent elections, rotation in office, and other limits upon power, and in at least one state what amounted to manhood suffrage. Framers of these laws and constitutions were forced to confront long-standing inequities which had aroused the populace for different reasons and in different areas. Impressive strides were made in correcting the inequitable representation of the interior and upcountry. States moved the burdens from poor to rich or from debtor to creditor either by issuing copious amounts of paper currency while postponing taxes necessary to support it—a lesson some

[72] See Yehoshua Arieli, *Individualism and Nationalism in American Ideology* (Cambridge, Mass., 1964), pp. 22, 23.

[73] Most recently, this thesis has been argued by Kenneth Silverman, *A Cultural History of the American Revolution* (New York, 1976).

[74] David Ramsay, *History of the American Revolution* (London, 1793), 2:315 ff.

[75] Morris, *The American Revolution Reconsidered*, 162. See, more recently, Olton, *Artisans for Independence*, 120.

[76] Edmund S. Morgan, "Conflict and Consensus in the American Revolution," in Kurtz and Hutson, eds., *Essays on the American Revolution*, 289–309.

[77] See Edward Countryman, "The Problem of the Early American Crowd," *Journal of American Studies*, 7 (1973):88–89, with suggestive leads to localized studies.

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of our modern cities seem to have mastered—or by overhauling the tax structure, shifting from polls to property, or from equal acreage to ad valorem taxes. The reformers made perfectly clear that they wanted these tax reforms to strike at the power "in the hands of the rich and chief men exercised to the oppression of the poor."[78] Small wonder that a troubled Landon Carter or a perturbed Charles Carroll of Carrollton should voice fears that the reforms proposed in their respective states would result in a government "independent of the rich men" and clearly "levelling" in their aims.[79] Since taxation precipitated the American Revolution, it is understandable that the colonial tax structure should be overhauled when Britain no longer ruled.[80]

In a society overwhelmingly agrarian, land was the measure of wealth and opportunity. A reformation of real-property law to curb monopolies and encourage equality was seen by Jefferson to be essential to reform. Jefferson's real-property reforms were at the very least a symbolic blow against a class-structured society, a blow emulated in most of the thirteen states, while it achieved a liquidity of assets which an indebted planter class so desperately needed. England waited until 1925 to pass the Real Property Act and thereby reach the point to which Jefferson had brought Virginia by 1776.[81] That gap of one hundred and fifty years marks the contrast between a society that achieved democracy by a revolution and one that reached it by glacial stages.

Confiscation of Loyalist estates, carried out in America with far more rigor than was to be the case in France,[82] enlarged the freeholding class. Granted that the initial purpose of forfeitures was fiscal rather than social, evidence suggests that rural holdings were extensively subdivided—immediately by exercise of pre-emption rights by tenants on large estates or later by purchases from speculators. The long-range effect of the program was indubitably egalitarian.[83]

If those targeted for confiscation proved to be landowners so imprudent as to have chosen the wrong side in the war, instead of all the big landowners, evidence exists that huge land monopolies were anathema to some Patriot reformers. Loyalists warned against the leveling that would follow independ-

[78] "Instructions to the Delegates from Mecklenburg to the Provincial Congress at Halifax in November, 1776," William L. Saunders, ed., *The Colonial Records of North Carolina* (10 vols., Raleigh, 1968), 10:870a–870f.

[79] Landon Carter to George Washington, May 9, 1776, in Force, ed., *American Archives: Fourth Series*, 6:390.

[80] See Robert A. Becker, "Revolution and Reform: An Interpretation of Southern Taxation, 1763 to 1783," *WMQ*, 3d ser., 32 (1975):417–42; for Maryland, Hoffman, "The 'Disaffected'," in Young, ed., *American Revolution*, 280–82, 307–08.

[81] Richard B. Morris, *Studies in the History of American Law* (New York, 1930), 123, 124. For a perceptive critique of the thesis of C. Ray Keim ("Primogeniture and Entail in Colonial Virginia," *WMQ*, 3d ser., 25 [1968]:585–86), see Berthoff and Murrin, "Feudalism, Communalism, and the Yeoman Freeholder," in Kurtz and Hutson, eds., *Essays on the American Revolution*, p. 283n. For a New Englander's affirmative evaluation of the egalitarian impact of partible descent and the abolition of entails, see Timothy Pickering to Charles Tillinghast, December 24, 1787, Massachusetts Historical Society, *Proceedings*, 5 (1862):413.

[82] See Georges Lefebvre, *The Coming of the French Revolution*, trans. by R. R. Palmer (Princeton, 1947), 134, 135.

[83] Compare Beatrice G. Reubens, "Pre-Emptive Rights in the Disposition of a Confiscated Estate: Pittsburgh Manor, New York," *WMQ*, 22 (1965):435–56; Catherine S. Crary, "Forfeited Loyalist Land in the Western District of New York: Albany and Tryon Counties," *New York History*, 35 (1954):329–58; Richard D. Brown, "The Confiscation and Disposition of Loyalists' Estates in Suffolk County, Massachusetts," *WMQ*, 3d ser., 21 (1964):549.

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ence. The best evidence was a provision in the draft Constitution of 1776 for Pennsylvania which the radicals were unable to have incorporated in the final document. It warned that "an enormous Proportion of Property vested in a few Individuals is dangerous to the Rights, and destructive of the Common Happiness of Mankind; and therefore every free State hath a Right by its Laws to discourage the Posesssion of such Property."[84] Rejected or not, this principle governed the seizure of the proprietary lands in Pennsylvania. "It was taken from the Proprietarys, not in a way of confiscation," one correspondent reminded Lady Juliana Penn in the summer of 1782, "but upon principle of policy and expedience. . . . They thought the estate two [*sic*] large for a subject to possess, supposing it dangerous to the public that so much property should rest in the hand of one family."[85]

In doubling the territory of the new United States, the Peace of Paris directly contributed to the process of democratization which was already under way in America, and held out a vision of boundless resources which only recent circumstances have forced Americans to modify. Vast unoccupied lands were now available to war veterans, landless New Englanders, and speculators. Consider that, from a population of some twenty-five thousand on the eve of the Revolution, the area west of the mountains increased fivefold in less than seven years. Settlement of the West proved perhaps to be the most important social movement to emerge from the American Revolution. This migration of the dissatisfied tempered frustrations and reduced social tensions. To the extent that the westward movement incorporated notions of squatter sovereignty, the borderers invested their settlements with decided social overtones.[86]

No review of the socioeconomic dimension of the American Revolution can afford to ignore the wartime debtor-creditor confrontation between tobacco planters and English and Scottish merchants. Many Virginians, as George Mason reminded Patrick Henry, fought the war to get rid of these debts.[87] If the British creditors were tenacious in pressing the besieged government at home for their pound of flesh, the American debtors employed every legal dodge to evade payment. This brought a swarm of hornets about the ears of the federal court judges, their repeated decisions in favor of creditors notwithstanding. As minister plenipotentiary to Great Britain, Chief Justice Jay found it expedient to strip the Supreme Court of final jurisdiction over such issues, and the treaty which bears his name permitted appeals to a mixed commission. The United States would assume payments for all debts validated by the commission. Thereby many debtors managed to socialize their debts, even though the government succeeded in scaling down its obligations.

[84] *The Proceedings Relative to the Calling of the Conventions of 1776 and 1790* (Harrisburg, 1825), 54–57. See also Eric Foner, *Tom Paine*, 133.

[85] James Tilghman to Lady Juliana Penn, Chester Town, Maryland, August 14, 1782, Shelburne Papers, 72:311, W. L. Clements Library, University of Michigan, Ann Arbor.

[86] See Francis S. Philbrick, *The Rise of the West, 1754–1830* (New York, 1965), 99.

[87] George Mason to Patrick Henry, May 6, 1783, in William W. Henry, *Patrick Henry: Life, Correspondence, and Speeches* (New York, 1891), 2:187.

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The issue of the planters' debts is perhaps the best example of how the Revolution redistributed liabilities rather than assets.[88]

The case for significant social change during the Revolution still needs to be made. One could point to the insolvency laws, to the democratization of education, and to church disestablishment and religious liberty legislation. Indubitably, reform in these diverse categories helped create a more egalitarian and pluralistic society.

In no area is the social effect of the American Revolution more visible than in the opportunities for new men to enter government, business, and the professions. The Revolution brought all the "dregs" to the top, complained a Philadelphia grandee.[89] Some who enjoyed a precipitous rise capitalized on the special opportunities the Revolution afforded in privateering, war manufacturing, and provisioning, on the new trade patterns resulting from the war, and on speculative opportunities provided by wartime and postwar inflation. A large body of statistical evidence is now available to show how state legislatures were altered to the advantage of newly settled areas and of men of less-established families. Statistics document some displacement of the old colonial "upper" class.[90] James Madison, without benefit of a computer, had long before reached this same conclusion. In his Sixty-second *Federalist* he stressed "the mutability in the public councils," which he attributed to "a rapid succession of new members." "Every new election in the States," he said "is found to change one half of the representatives."

The biographical record also demonstrates how the Revolutionary War brought a transformation in politics, business, and the professions. Consider that populist prototype, the New Yorker Abraham Yates, Jr., always an object of venom among the Federalists, who reserved for him choice epithets ranging from the "late cobbler of laws and old shoes" to "an old booby." Apprenticed to a shoemaker, he became a lawyer, and as sheriff allied himself with Robert Livingston, Jr. in the skirmishes against the so-called tenant rebels. A central figure in putting the new state government into operation, he proved both in Congress and in the state legislature an unreconstructed anti-Federalist. Or take the Irish redemptioner Matthew Lyon, whose pugnacity, enterprise, and leadership (not to speak of an influential second marriage) elevated him within a decade after war's end to an established position in his region, even if his affluence failed to render some of his coarser habits acceptable. That orphaned backwoodsman Andrew Jackson, who would spend more time on horseracing and cockfighting than on Blackstone, was admitted to practice in 1788 after two years of haphazard tutelage, adjudged

[88] See Richard B. Morris, *John Jay, the Nation, and the Court* (Boston, 1967), 73–97.

[89] "Diary of James Allen, Esq., of Philadelphia, Counsellor at Law, 1770–1778," *Pennsylvania Magazine of History and Biography*, 9 (1885):192.

[90] Jackson Turner Main, "Government by the People: The American Revolution and the Democratization of the Legislatures," *WMQ*, 3d ser., 23 (1966):391–407; *The Upper House in Revolutionary America, 1763–1788* (Madison, Wisc., 1968); *Political Parties Before the Constitution* (Chapel Hill, 1973), for the lower house. See also James Kirby Martin, *Men in Rebellion: Higher Governmental Leaders and the Coming of the American Revolution* (New Brunswick, N. J., 1973), 195–96. For the advance of the common man in Philadelphia office-holding, see Olton, *Artisans for Independence*, 80.

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by the court to be "a person of unblemished moral character, and competent . . . [in] knowledge of the law." And why not Henry Clay? That barefoot boy of the Slashes in old Hanover was left by his mother at the age of fifteen in the office of the Virginia Court of Chancery. As he recalled it, he started his practice in Lexington in 1797 "without patrons, without favor or countenance of the great or opulent [and] without the means of paying my weekly board." Jackson's and Clay's was a vastly different era from the prewar years. A transformed society had spawned a new breed of professionals and politicians.[91]

A people's revolution achieved more than independence and nationhood. It brought new men to power, raised people's political aspirations, made the new governments of the Revolution more responsive to social inequities, and underpinned the notion of the sovereign people as the constituent power, of which the Preamble of the Federal Constitution is the most eloquent affirmation.

[91] Alfred F. Young, *The Democratic Republicans of New York: The Origins, 1763–1797* (Chapel Hill, 1967), 44, 45; Anton-Hermann Chroust, *The Rise of the Legal Profession in America* (Norman, Okla., 1965), 2:38; Calvin Colden, ed., *The Life, Correspondence and Speeches of Henry Clay* (New York, 1857), 1:29; Aleine Austin, "Matthew Lyon, 'New Man' of the Democratic Revolution: His Early Career, 1749–1801" (Ph.D. dissertation, Columbia University, 1970).

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# Robespierre and the French Revolution

A Review Article by JOSEPH I. SHULIM

MARC BOULOISEAU. *La république jacobine, 10 août 1792–9 thermidor an II.* (Nouvelle histoire de la France contemporaine, number 2.) Paris: Éditions du Seuil. 1972. Pp. 290.

MAX GALLO. *Maximilien Robespierre, Histoire d'une solitude.* (Présence de l'histoire. Collections historiques.) Paris: Librairie Académique Perrin. 1968. Pp. 352. English translation: *Robespierre the Incorruptible, A Psycho-Biography.* Translated by RAYMOND RUDORFF. New York: Herder and Herder. 1971. Pp. 336.

NORMAN HAMPSON. *The Life and Opinions of Maximilien Robespierre.* London: Duckworth. 1974. Pp. xii, 313. £5.95.

ALBERT MATHIEZ. *Études sur Robespierre (1758–1794).* Preface by GEORGES LEFEBVRE. (Société des études Robespierristes.) Paris: Éditions sociales. 1973. Pp. 280.

BRUCE MAZLISH. *The Revolutionary Ascetic: Evolution of a Political Type.* New York: Basic Books. 1976. Pp. viii, 261. $11.95.

GEORGE RUDÉ. *Robespierre, Portrait of a Revolutionary Democrat.* New York: Viking Press. 1975. Pp. 254. $10.95.

ALBERT SOBOUL. *The French Revolution, 1787–1799, From the Storming of the Bastille to Napoleon.* Translated by ALAN FORREST and COLIN JONES. New York: Vintage Books. 1975. Pp. 639. Paper $5.95.*

WHILE GEORGE RUDÉ IS CORRECT in holding that no major study of Maximilien Robespierre has appeared in English since J. M. Thompson wrote his two-volume biography in 1935, he exaggerates when he asserts that interest in Robespierre has been at a low ebb in the English-speaking world since then and until recently.[1] Every decade since the 30s has seen some important work in English on Robespierre written from one aspect or another: R. R.

* I have not included reviews of two recent works on Robespierre: 1.) John Lawrence Carr's *Robespierre, The Force of Circumstances* (New York: St. Martin's Press, 1972) is a combination of careful scholarship at some points with the acceptance of unreliable material at others. Some of his interpretations are so extreme as to violate all the canons of historical scholarship; 2.) Jean Matrat's *Robespierre or the Tyranny of the Majority*, tr. Alan Kendall and Felix Brenner (New York: Charles Scribner's Sons, 1975) is a poor example of popular history, compounded by a poor translation. See my review of Matrat's book in *History: Reviews of New Books*, 4 (1975): 6–7.

[1] George Rudé, *Robespierre, Portrait of a Revolutionary Democrat*, 9.



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Palmer's study of the Committee of Public Safety and Alfred Cobban's articles on Robespierre's political thought in the 1940s, J. L. Talmon's *Origins of Totalitarian Democracy* in the 50s, and so forth.[2] True there has been a flurry of publications in English dealing with Robespierre in the late 60s and in the 70s, but no full-length biography by a specialist in the French Revolution. Neither Rudé nor Norman Hampson has attempted this in their new studies.

Rudé's section on the historiography of Robespierre is a sensible, brief survey of changing attitudes. He is especially good on the factors that helped shape the image of Robespierre. He traces the developing rehabilitation in the nineteenth century—despite continuing hostile accounts—with its apex in mid-century with Ernest Hamel's biography.[3] Another peak was reached in the twentieth century with Albert Mathiez. Since World War II, however, the interpretation of Robespierre has become more complicated and generally less adulatory and more critical. Major forces in this change have been Marxism and the study of history "from below," thereby diminishing "the cult of personality" (p. 88).

Rudé believes that both Gérard Walter and Crane Brinton came under the influence of Mathiez (pp. 80–81). He exaggerates. Walter at times gives quite an unfavorable interpretation of Robespierre in his pre-1789 years and after.[4] Brinton was not at all a follower of Mathiez. He not only declared in *A Decade of Revolution* (1934) that Mathiez was wrong but presented an extremely hostile portrait of Robespierre.[5] Brinton, furthermore, took no position on the conflicting interpretations of Alphonse Aulard and Mathiez concerning the Girondin-Montagnard struggle. Indeed, Brinton misunderstood "the economic interpretation of history,"[6] which he confused with a Beardian economic determinism.[7]

Rudé concludes his historiographical section with the statement that "whether recent historians have been personally attracted to Robespierre or not, they are almost unanimous in accepting Mathiez's verdict that he was the outstanding figure of the French Revolution" (p. 92). While this may be putting the case too strongly, it is essentially correct.

Students of the French Revolution will be pleased, therefore, that Albert Mathiez's scarce volume of miscellaneous articles on Robespierre, *Études sur Robespierre*, has now been reprinted. The rehabilitation of Robespierre by Mathiez, based as it was on research in primary sources, remains the foundation of current Robespierriana. Mathiez hoped that his establishment of the

[2] R. R. Palmer, *Twelve Who Ruled, The Year of the Terror in the French Revolution* (reprint; New York, 1965). Alfred Cobban, "The Political Ideas of Maximilien Robespierre during the Period of the Convention," *English Historical Review*, 61 (1946): 43–80; and "The Fundamental Ideas of Robespierre," *EHR*, 63 (1948): 29–51. J. L. Talmon, *The Origins of Totalitarian Democracy* (London, 1952).

[3] Rudé, following Jacques Godechot ("L'historiographie française de Robespierre," *Actes du colloque Robespierre*, XII[e] Congrès international des sciences historiques, Vienne, 3 septembre 1965 [Paris, 1967], 179–80), is unduly critical of Hamel's work; see *Robespierre*, 71–72.

[4] Walter was correctly critical of Mathiez at times. See, for example, his *Robespierre* (édition définitive; Paris, 1961), 1: 27, 30.

[5] (New York, 1963 ed.), 107–09.

[6] *Ibid.*, 131.

[7] See, for example, his *Anatomy of Revolution* (New York, 1968), 29–30, 33.

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Société des Études Robespierristes and the *Annales révolutionnaires* (since 1924 the *Annales historiques de la Révolution française*) would perpetuate his work. The *Études sur Robespierre* first appeared in 1958 on the fiftieth anniversary of the Société and of its journal, as well as the bicentenary of Maximilien's birth. Though they contain essays published originally between 1910 and 1924, they still retain their freshness. Mathiez's impassioned tone and even occasional eloquence are striking, as he struggles to rehabilitate his hero, to criticize Danton (the hero of his former mentor, Aulard) whenever possible, and to attack contemporary politicians and their policies through the eulogy of Robespierre. This master of polemical history was continuing Robespierre's campaign for a moral and democratic France. Mathiez's skill as a historian was displayed in this volume, for example, in his critical editions of Robespierre's *carnet* and of Robespierre's notes against the Dantonists, and in his attempt to reconstruct the events of 9 Thermidor.

The Robespierre of Mathiez is well known to specialists in the Revolution. Inspired by Rousseau, the incorruptible, practical, patriotic statesman strove to establish a democratic republic based on *liberté, égalité, fraternité,* and *justice.* He employed *la terreur* only because of the emergency and with great moderation, hoping to end it soon. The enemy of the bourgeoisie and the champion of the humble and disinherited, Robespierre was the harbinger of modern socialists.

Mathiez's idolization of Robespierre as well as his concern with practical politics led him into errors. Historians, furthermore, have learned a good deal about the *ancien régime,* the Revolution, and Robespierre since Mathiez's untimely death in 1932. One can, for example, no longer accept Mathiez's account of Maximilien's address while at Louis-le-Grand to Louis XVI and Marie Antoinette, or Maximilien's role in the lightning-rod case. Nor can one accept his view of Robespierre as one of the greatest orators who ever lived or as a virtual socialist, admired by Babeuf even before Thermidor.[8] Mathiez's terminology was sometimes as loose as Robespierre's. Mathiez equated the proletariat with the sans-culottes and the "people." From the *Études* one gains the impression at times that for Mathiez socialism was merely democracy with a concern for the humble and disinherited. Despite these weaknesses and others, this volume, as well as many other writings of the father of modern Robespierriana, must still be read before undertaking any reappraisal of that revolutionary leader.

Influenced by Jean Jaurès, Mathiez developed an economic interpretation of the Revolution, including the struggle between the Gironde and the Mountain. This struggle, he maintained in other writings, became almost a class conflict. The Girondins included a large number of property owners, who felt a repugnance for the coarse "people." They believed that property was sacrosanct, that the bourgeoisie should rule, and they adhered to a thoroughgoing economic liberalism. The Mountain represented "*les petites gens.*" More

[8] On Babeuf, see R. R. Palmer, *The Age of Democratic Revolution, A Political History of Europe and America,* vol. 2: *The Struggle* (Princeton, 1964), 237–38.

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realistic and less attached to theories than the Girondins, they recognized that the emergency required extraordinary measures. They readily supported the right to a livelihood against the right to property, and public as against individual interests. "In case of need they were prepared to have recourse to restrictions upon individual liberty and property, if the superior interests of the masses required it." But "for many of the Montagnards . . . the policy of rapprochement and collaboration with the masses was primarily a tactical move rendered necessary by the exigencies of the war." Most of the Montagnards, like the Girondins, were of bourgeois origin. "The class policy which they inaugurated had not sprung directly from the people. It was a policy required by circumstances, a plebeian method, as Karl Marx puts it, of doing away with kings, priests, nobles—with all the enemies of the Revolution."[9]

The entrance of Robespierre into the Committee of Public Safety opened a new era. With his admirable personal qualities and his organizational ability, he brought also his influence as the undisputed leader at that time of the sans-culottes. Thermidor ended the era. After Thermidor—but alas, too late—the sans-culottes were to regret the passing of Robespierre. Their attempts to force the re-establishment of the Maximum in Germinal and Prairial of the Year III were to be crushed. Mathiez asseverated that Robespierre, Saint-Just, and Couthon had desired to prolong the dictatorship in order to establish by means of the Ventôse decrees the egalitarian republic without rich or poor. But Robespierre's uncompromising character was enough to lead to the collapse of a structure existing in a legal vacuum.[10] "A memorable example of the limitations of the human will in its struggle against the resistance of material things."[11] Mathiez thus believed that even a great leader's opportunities were limited by "*la force des choses*."

ALBERT SOBOUL, A MORE ORTHODOX MARXIST, inevitably shifted the emphasis in his analysis of the Revolution from individuals to "class struggles." *The French Revolution, 1787–1799*, which originally appeared in Great Britain in 1974, is the English translation of his *Précis d'histoire de la Révolution française* (1962) with the addition of a foreword. According to Soboul, the French Revolution, the "classic model of bourgeois revolution" (p. 1), was also a revolution of peasants and popular masses. His analysis of the struggle between the Gironde and the Mountain resembles that of Mathiez and Georges Lefebvre. The Girondins, writes Soboul, were representatives of "the commercial and industrial bourgeoisie, whose interest it was to defend property rights and economic liberty against the restrictions demanded by the

[9] Albert Mathiez, *La Révolution française*, vol. 2, (4th ed.: Paris, 1932), 67–69. *The French Revolution*, tr. Catherine Alison Phillips (New York, 1964), 212–13. See also Mathiez, *Girondins et Montagnards* (Paris, 1930), ch. 1 (a paper read in 1923).

[10] See Mathiez, *La Révolution française*, vol. 3, (3d ed.; Paris, 1930), 222; *The French Revolution*, 309; and ch. 5 of *Girondins et Montagnards*.

[11] Mathiez, *The French Revolution*, 310.

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sans-culottes" (p. 276). The Montagnards represented "the middle bourgeoisie and the popular classes of society" (p. 277). The Montagnards understood that the crisis which faced France demanded exceptional measures and that these measures required the support of "the people." They were more realistic than the Girondins, being closer to the people and their needs; they were prepared to allow the public interest to take precedence over their own private interests.

While Mathiez's sympathies were primarily with the Revolutionary Government, however, Soboul, the authority on the sans-culottes movement in Paris, sympathizes with the sans-culottes and stresses their leadership in demanding emergency measures, including economic regulation. He attributes the fall of the Mountain to its "internal contradictions" (e.g. pp. 281, 328, 335). The Jacobins fundamentally were still bourgeois, and the antagonism between them and the sans-culottes was "unbridgeable" (pp. 372, 383). Even during the Year II the bourgeoisie did not lose control of the Revolution (pp. 414–15).

What happens to Robespierre in Soboul's dialectical materialist analysis of the Revolution? Speaking about "The Hero and History" at a colloquium on Napoleon, Soboul maintained that Napoleon's influence depended on the degree to which he followed the currents of history.[12] His discussion of Robespierre in the *French Revolution*, consequently, is subordinated to his analysis of trends in the "class struggle." Robespierre is incidental to the history. Nevertheless, Soboul, having helped edit the *Discours* of Robespierre for the Société des Études Robespierristes, is well acquainted with the man.[13]

Soboul's opinions of and attitudes toward Robespierre seem ambivalent. On the one hand he describes Robespierre as one of the more important democratic deputies in the Constituent Assembly, generally "defending the rights of the people with great consistency and perspicacity" (p. 215). He lauds Robespierre's opposition to the war (p. 239). On Robespierre's election to the Committee of Public Safety, Soboul writes: "Because of his reputation as a revolutionary, Robespierre was able to impose the policies of the Committee on the Convention and on the Jacobin Club. Farsighted and courageous, eloquent and disinterested, he was the only man in French history to merit the description of 'incorruptible,' and he enjoyed the confidence of the sans-culottes. Though committed to his principles, he was nevertheless capable of moulding his policies to the needs of the situation and was statesmanlike in his political manoeuvres" (p. 325). Soboul repeats Mathiez's interpretation that Robespierre's theoretical statements subordinated the right to property to the right to existence (pp. 15–16). The ideal of social democracy was shared with some difference of interpretation by both the popular classes and the middling bourgeoisie, who formed the revolutionary leadership and who were "vividly personified by Robespierre (p. 281). Their common ideal was a society of small producers. Robespierre and his

[12] "Le Héros et l'Histoire," in *La France à l'Époque Napoléonienne* (*Revue d'histoire moderne et contemporaine*, special issue, vol. 17 [July–September 1970]), 333–38.

[13] *Oeuvres de Maximilien Robespierre* (hereafter *Oeuvres*) (Paris, 1950–67), vols. 6–10.

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supporters gave this ideal more detailed expression. Several random statements of Robespierre were taken by Mathiez and Lefebvre, as well as Soboul, to prove Robespierre a social democrat.

On the other hand, Soboul notes that Robespierre said nothing about the Le Chapelier Law. Only a later pro-labor or Marxist historian could see this as the result of an antiunion bias rather than give it a more likely explanation such as the acceptance of the prohibition of any corporate bodies which had characterized the *ancien régime*. Soboul, apparently, contradicting his assertions elsewhere in the book, says that Robespierre's proposed declaration of rights of April 24, 1793, which would have made property subordinate to social utility, was essentially a tactical move to defeat the Girondins and give the sans-culottes hope that the Jacobins were moving toward social democracy (pp. 307–08). From August 1793 on, Robespierre attacked the *enragés*, "not without a degree of bad faith" (p. 327). Robespierre and the other members of the Committee of Public Safety were not sure of the means that should be used to deal with the emergency. The extraordinary measures were forced on the leadership by outside forces whether as a result of the crisis of August 1793 or of pressure from the popular movement (p. 326). With regard to the Ventôse decrees, Soboul—following Lefebvre[14]—correctly notes that, despite Mathiez's beliefs, they were not, as finally drafted, significant social measures. The decrees, Soboul writes, were incapable of solving the agrarian problem. "For like the [rest of the] Montagnards, Robespierre and his friends were at heart believers in economic freedom. . ." (p. 396).

The truth is that economics was of little import in Robespierre's thought. Even Soboul recognizes this in his treatment of the Worship of the Supreme Being. While essentially following Mathiez's interpretation of Robespierre's religious policy as primarily a tactical political move to unite the country, Soboul correctly notices the influence of Robespierre's religious beliefs (p. 398–99). The political objective was doomed to failure, however, in the face of "the class antagonisms" of the diverse groups which had hitherto supported the Revolutionary Government. "Unable to analyse economic and social conditions, Robespierre believed in the omnipotence of ideas and appeals to virtue" (p. 399). While this is intended as a Marxist value judgment of Robespierre, the facts are essentially correct.

"As a result of his outstanding services to the Revolution," Soboul writes, "Robespierre had become the real leader of the government in the eyes of the country. But he made no concessions to the susceptibilities of his colleagues" (p. 409). Like Saint-Just, Robespierre "remained the prisoner of his own contradictions; they were both too conscious of the interests of the bourgeoisie to give their total support to the sans-culottes, and yet too attentive to the needs of the sans-culottes to find favor with the middle classes" (p. 412). The Jacobins, furthermore, were neither a class nor a class party, strictly disciplined, which could have been an effective instrument for political action. The Mountain fell primarily because of its internal contradictions.

[14] *La Révolution française* (Peuples et Civilisations; new ed.; Paris, 1951), 398–99.

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Thus Soboul's apparent ambivalence toward Robespierre is a manifestation of his materialist interpretation. Robespierre stood between "the bourgeoisie" and the masses. His "internal contradictions" prevented his becoming a true champion of the sans-culottes. In any case, his role was subordinate to the more fundamental "class struggles."

With respect to the Girondin-Montagnard conflict, to describe it as a virtual class conflict is merely an attempt to fit it into the a priori Marxist interpretation. Soboul admits that there is still no history of the bourgeoisie during the French Revolution. He has no evidence to prove that the Mountain represented "the middle bourgeoisie." He further shows that the sans-culottes forced the Mountain to adopt the emergency economic measures and that later the chasm between them was unbridgeable. The Ventôse decrees, moreover, were of little importance. Thus, the "class struggle" vanishes. The conflict between the Girondins and the Montagnards becomes in reality a difference of opinion between revolutionists over strategy and tactics. The Montagnards, unlike the Girondins, agreed to make concessions to *les petites gens* in order to win the war and save the Revolution.

MARC BOULOISEAU, COEDITOR WITH GEORGES LEFEBVRE (till his death) and Albert Soboul of the *Discours de Maximilien Robespierre,* wrote a brief and now classic biography of Robespierre in the "*Que sais-je?*" series (Paris, 1956). Continuing the tradition of Mathiez and Lefebvre, it was an extremely sympathetic presentation of Robespierre's actions and thoughts. In essence Bouloiseau accepted Lefebvre's appraisal of Robespierre a few years later on the bicentennial of Maximilien's birth: "*Apôtre de la démocratie politique, il a fini par s'inscrire aussi, avec Saint-Just, parmi les protagonistes de la démocratie sociale.*"[15]

In writing *La république jacobine,* however, Bouloiseau's approach to Robespierre shifted—he removed Robespierre from the front stage of Revolutionary history. His main concern is history "from below," economic and social history, and the persistence of collective mentalities—the immobility of the masses—as against the attempts of revolutions to change conditions. Using his own archival research and that of others of the last twenty years, Bouloiseau provides a corrective to the earlier examination of the radical period of the Revolution from the vantage point of the events in Paris. The reader gets an interesting picture of the "conservatism" of rural France rejecting in the end the centralizing power of the Revolutionary Government, a "dictatorship" whose authority in the localities had previously been exaggerated. There is also a description of the revolutionary mentality, the mood of the Terror, and the development of the autonomy of the new army together with a new militarism. The volume, therefore, is more analytical than narrative history.

[15] Georges Lefebvre, "À la Mémoire de Maximilien Robespierre," in *Maximilien Robespierre, 1758–1794,* ed. Walter Markov (Berlin, 1961), 11. Cf. Lefebvre's opinion of Robespierre in 1933: G. Lefebvre, *Études sur la Révolution française* (2d ed. rev.; Paris, 1963), 144–48.

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*L'histoire événementielle*—to use the terminology of the *Annales* school—inevitably has to be discussed in a book on the Jacobin regime. It is handled briefly and with the skill of the specialist. Though Robespierre is mentioned, he seems, except for Thermidor, incidental to the history. Even the date of his entrance into the Committee of Public Safety is not given.

Bouloiseau retains a Marxist, and a patriotic and generally pro-Jacobin, approach in his interpretation of the period. Though he questions terms such as "bourgeois" and "peasantry"—they are too broad and conceal too many antagonistic interests (p. 11)—he soon slips back into asserting that "the bourgeoisie," at this point defined as "*les détenteurs du savoir*" or the means of production or both, continues to rule in the Jacobin regime (p. 12). The term "bourgeoisie" is used loosely throughout the book. After August 10, the author maintains, social antagonisms were vulgarized into a struggle between "rich" and "poor" (p. 34). Bouloiseau's sympathies are with the "poor."

The Gironde-Montagne rivalry is still seen as reflecting a division of the "bourgeoisies." Admitting that historians now know much more about the popular movement than that of the bourgeoisie, he does not find any difference in the social composition of the two in the Convention. (Of course, there is no proof of any such difference.)[16] Both, the author states, belonged to the same professional categories, shared a similar education, and had habits of life and thought which were very close. Bloodshed frightened neither. Their disagreements manifested themselves in attitudes more than in "*des impératifs doctrinaux.*" Their opinions diverged only progressively (pp. 58–59).

Bouloiseau maintains, however, that the Girondins were led by their electoral support to pursue a class policy. The rich rallied to them. Without belonging directly to the great merchants, shipowners, manufacturers, and financiers, the Girondins supported their views, not because they had a coherent policy, but out of conservatism and because of certain links with the Physiocrats. They considered landed wealth and commercial capital as the sole factors of social consolidation (p. 63). The clientele of the Mountain, Bouloiseau alleges, included the sans-culottes of the towns as well as the rural artisans and the small peasants. The Montagnard bourgeoisie—like the Girondins largely provincials, not Parisians—were more realistic and willing to restrict property to help the poor. The popular movement—strengthened by the subsistence problem and the war—forced the hesitant Mountain to take emergency measures to save the Revolution. Economic questions dominated the revolutionary struggle.

The Montagnards identified themselves with the Jacobins. Jacobinism mobilized national sentiments and egalitarian hopes. Robespierre would extol virtue in the Year II. The Jacobin conception of property, shared by the great majority of the people, stopped at a community of small independent producers, increasing in numbers, among whom the state, by its laws, would assure an approximate equality (pp. 42–43).

[16] See Alison Patrick's interesting treatment of this problem in *The Men of the First French Republic: Political Alignments in the National Convention of 1792* (Baltimore and London, 1972), 286ff. Patrick's book appared in the same year as Bouloiseau's.

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Although "the average Frenchman" occupied the stage of the Revolution from August 10 to Thermidor, Robespierre does step forward at crucial times and, as the author admits, influences events. A leader thus does seem to have a role in history despite the author's disclaimers in other parts of the book. Robespierre provided the philosophical justification for August 10 and for Revolutionary Government in his speech of July 29, 1792. After August 10, Robespierre gave the Paris Commune his experience. Robespierre's great speech of December 3, 1792, reflecting the sentiments of the sans-culottes, led the Convention to decide to try the king (p. 64). After September 4–6, 1793, the Jacobins (with Robespierre) succeeded in diverting the popular restlessness and preserving the alliance with the sans-culottes.

The patriotic Revolutionary Government was the work of sincere democrats. Billaud-Varenne, Saint-Just, and Robespierre furnished the revolutionary dictatorship with its theoretical foundations. To the great Committee of Public Safety Robespierre brought his experience and political sense. After the fall of the factions, the dictatorship treated its citizens as subjects, aiming to discipline a popular movement which threatened its unity. The trial of the Hébertists was a parody of justice. The Jacobin dictatorship, by strengthening itself, condemned the Revolution.

The attempt of the central government to impose its authority on the peasantry was blocked by the rigidity of the rural masses. Dechristianization angered many of them. Following Mathiez, Bouloiseau asserts that Robespierre was not the only one or the first to suggest a system of national fêtes. The report of 18 Floréal merely codified them. Following Lefebvre,[17] Bouloiseau notes that the Convention had no agrarian policy. Though Mathiez was wrong in considering the Ventôse decrees an innovation, he writes, the Robespierrists had the courage to get them adopted. Most certainly, states Bouloiseau, Couthon and Robespierre, though they did not say anything, agreed. (There is, nevertheless, no evidence for this or for the traditional conception of a Robespierrist triumvirate in the Committee of Public Safety.) The Convention, by changing Saint-Just's proposal for a free distribution of land into an ambitious plan of national charity, the author concludes, made the decrees deceptive.

Thermidor, according to Bouloiseau, was the result primarily of the unconscious resignation of Jacobinism. The number of malcontents increased. After Germinal, the government inaugurated a policy favorable to the men of property. The sans-culottes and the workers were disgruntled. Disarray showed itself at the Jacobin Club. Robespierre made himself the defender of the Jacobin order. He repeated his old ideas, was listened to with respect, but he was no longer convincing. The reign of the bureaucrats resulted in superficial conformism. The Terror paralyzed opinion. The recall of the terrorist representatives on mission, sincere Montagnards, created new enemies for the government. As Lefebvre had noted, Bouloiseau declares, the crisis of Ventôse-Germinal reacted on the collective mentality in the fashion of the Great

[17] Lefebvre, *La Révolution française*, 395ff.

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Fear of 1789. And the application of the Ventôse decrees, which Mathiez may have overstressed, says Bouloiseau, was a factor in creating disillusionment.

Robespierre bore the brunt of the national criticism. His mood was one of despair. Yet Robespierre objected to the Great Terror—not because he wanted to moderate the Terror, but because, "with a keen political sense," he realized the dangers of excessive violence (pp. 232–33). In any case, the Terror appeared to the public to be off the track. There was a general malaise in society. The results of the Maximum were more profound in creating it among the peasantry than the excesses of dechristianization and the executions. A point of rupture had been reached between a conservative society and its artificial government. The government operated more and more in a void, and as soon as the rulers split, it collapsed.

Insufficient attention, continues Bouloiseau, has been given to personal conflicts, to exhaustion, etc. Robespierre left the field to his adversaries. He was bound by his scruples. As a sincere democrat he could not rebel against the sovereign Convention. He acted as chief of the executive, asking for a vote of confidence. He thus gave credence to the accusations of dictatorship. Robespierre, a fatalist and conscious of his powerlessness, accepted his fate. For the first and only time in the book the author treats Robespierre as a person, not just a name. It is unfortunate that Bouloiseau stops short of attempting to understand Robespierre's psyche.

In conclusion, Bouloiseau declares that the First Republic had no model; it was the response to circumstances. "The bourgeoisie of office, land, and commerce" (p. 251) was able temporarily to control the direction of popular action. The dictatorship was intransigent and sometimes cruel. Its social measures did not satisfy the poor and frightened the rich. The state met the opposition of established structures and mentalities. The Year II, thus, could only be temporary. Its only permanent achievement was its army. The miracle of the Year II was in making the social plans—arising from popular thought—which it promised and which France has not yet attained. This was an exceptional period in the Revolution and French history. François Furet and Denis Richet (members of the *Annales* school) are wrong in considering the Year II as "an unfortunate and useless deviation." Despite Germinal, it was "*la phase ascendante de la lutte pour la Liberté*" (p. 12).

The book contributes to Robespierriana by observing the leader from below and thereby restoring balance to historical interpretation, which tended in the past to overemphasize the leader at the expense of the society. By moving too far to the other extreme, however, Bouloiseau fails to give a complete picture of the period. In other words—to use *Annales* terminology—he does not connect adequately and fully enough the long-term structures and trends with the history of *conjonctures* and *événements*. It is true, as the author notes, that information is insufficient on many aspects of this interaction. A comprehensive picture of the Year II, nevertheless, would combine—in so far as possible—the study of the revolutionary leader with the study of society in all its manifestations.

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WITH GEORGE RUDÉ'S *Robespierre, Portrait of a Revolutionary Democrat* the emphasis shifts back to the leader. The author declares, however, that this is a political portrait, not a personal biography. Uninterested in Robespierre's personality, he alleges that it is of minor relevance to the study of a political leader and revolutionary. This is a debatable assumption. How is it possible to understand a revolutionary, for example, if we do not know his personality traits and how they were influenced by the environment to compel him to break with the status quo? Rudé has to admit, when examining the reasons for Thermidor, that there were "weaknesses" in Robespierre's character which must be included in the explanation (p. 45).

The author states that though he has consulted Robespierre's own works and used some of his research findings on the Revolutionary crowd, the book is essentially one of synthesis rather than original research. The first part, written for the nonspecialist, presents a brief survey of Robespierre's life and of the Revolution. It is followed by the historiographical section. Then part 3 examines the thought of Robespierre, with one chapter attempting to prove that he was a political democrat and the second that he was also a social democrat. The influence of Mathiez and Lefebvre is therein evident. The last part aims to demonstrate that, with some exceptions, Robespierre was a realistic, revolutionary politician, a popular leader, and, despite a number of shortcomings, an outstanding revolutionary leader. The result is a sympathetic political portrait of Robespierre without the adulation of Mathiez.

Though Rudé adheres basically to a Marxist interpretation of the Revolution, and notwithstanding his great interest in history "from below," he gives Robespierre a greater role in historical development than do other Marxists. Rudé concedes that a leader does influence historical developments up to a point. He is limited, however, by the nature of the times in which he lives. The leader represents (or unleashes) social forces (pp. 87–88). Yet he does have some freedom of choice in other areas. In one such area—Robespierre's tactics on the eve of Thermidor—Rudé avoids an explanation or attempted explanation of his errors. Since the author does not study Robespierre's personality and doubts the possibility of analyzing Robespierre's psyche, he has to avoid seeking an answer to the personal equation. Again, Rudé has not written a biography of Robespierre.

The reader sometimes gets the impression that the book was rushed to completion. This haste is shown in a number of inaccuracies and contradictions. Here are a few examples: Rudé quotes an alleged speech by Robespierre on June 6, 1789, as reproduced by J. M. Thompson.[18] But Walter proves that this is a false attribution, and the editors of Robespierre's *Discours* agree with Walter.[19] Similarly, relying again on Thompson,[20] Rudé writes that Robespierre argued on September 7, 1789 against the royal veto by invoking Rousseau's "law of the General Will." Thompson was later proven

[18] See Rudé, 19, and J. M. Thompson, *Robespierre* (Oxford, 1935), 1:50–51.
[19] See Walter, *Robespierre*, 1:82ff., and *Oeuvres*, vol. 6, *Discours, 1789–1790*, 29–31.
[20] Rudé, 21; Thompson, *Robespierre*, 1: 58–59.

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incorrect by the editors of the *Discours*, who showed that Robespierre, unable to deliver the lengthy address he had prepared, published it as a pamphlet in late September. If Rudé had checked this *Diré . . .* of Robespierre in the *Oeuvres* (6:86–95), he would have seen that, contrary to Rousseau's political philosophy, Robespierre maintained that the will of the representatives was the will of the nation and was superior to all individual wills. While Rudé goes so far as to assert that "no other revolutionary leader identifies so closely with Rousseau in both word and deed" (p. 97), he continues that Robespierre did not slavishly follow Rousseau and adapted Rousseau's ideas to suit the occasion and, further, that he only followed Rousseau when it accorded with the policy he had decided to pursue (pp. 98, 100). A combination of the last two interpretations seems to this reviewer to be the most accurate one. In other words, Robespierre was a pragmatist.

Because of the topical organization, the book, as the author himself acknowledges, is frequently repetitious. The same events, treated from different angles, are discussed more than once. Sometimes sentences are repeated almost verbatim. The chapter entitled "The Popular Leader" says very little that has not already been covered. It could easily have been integrated into earlier chapters.

Rudé's description of Maximilien's life in Arras minimizes his legal career; he lived modestly, says Rudé, as a poor man's advocate. On the contrary, Robespierre's practice was successful. His clients included advocates, the superior of the Oratorian *collège*, and the widow of an English lieutenant-governor of South Carolina, as well as a poor rope-maker and a seamstress's brother. He lived with his sister Charlotte from 1783 to 1789 in comfortable quarters. He dressed like a gentleman and signed his name "de Robespierre." Maximilien's brief in the lightning-rod case gave him national prominence. His winning a prize in the essay contest of the Academy of Metz and his leadership in the Arras academy must also be noted. Rudé oversimplifies Maximilien's youth.[21]

Rudé tends to give Robespierre a more radical theoretical position and a more activist role on a number of occasions than the evidence warrants. For example, concerning the flight to Varennes, I would emphasize the complexity, moderation, and ambiguity of Robespierre's statements in June–July 1791. On July 13, for example, he described "republican" as a vague word that could be applied in a thousand ways.[22] The form of a republic had hitherto not meant much to him.

Rudé, furthermore, following Mathiez and Lefebvre, tries with the same limited evidence they used to prove Robespierre a social democrat, aiming to create a country of small and middling producers. Yet Rudé does admit that, since Robespierre tended to temporize in social matters, we cannot be completely sure of his firmness on the social question. But "the logic of events

[21] Cf. Joseph I. Shulim, "The Youthful Robespierre and his Ambivalence toward the Ancien Régime," *Eighteenth-Century Studies*, 5 (1972): 398–420.

[22] *Oeuvres*, vol. 7, *Discours (2e partie) Janvier–Septembre 1791*, 551–52; July 13, 1791 at the Jacobins.

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and the exigencies of war" persuaded him of the need to act (p. 137). Rudé, however, later notes that Robespierre played no distinct part in the establishment of the Maximum (p. 139).

While in his introductory survey at the beginning of the book Rudé asserts that the Ventôse decrees were inspired by Robespierre and Saint-Just (p. 39), he later admits that those decrees were essentially prompted by the political needs of the moment and, in any case, Robespierre had little to do with them (pp. 141–42). Robespierre's thought was limited by the society in which he lived. His vision of the ideal society, the author states, reflected that of the small peasants and sans-culottes, a dream impossible of realization in an emerging capitalist country. Rudé insists that Robespierre deserves the reputation of having been a founder of French social democracy (pp. 142–43). If so, it was in spite of himself. As I have already noted, for Robespierre political and moral, not economic questions were uppermost in his thought.

Rudé is on stronger ground when he shows that Robespierre was a political democrat, despite the establishment of the Revolutionary Government. The author has a problem, however, as to whether Robespierre did not in the end lose his faith in democracy. He opts for the conclusion that Robespierre's faith in the ultimate triumph of his cause remained unshaken. To answer this problem more convincingly would require a study of Robespierre's personality and psyche, and this Rudé has eschewed.

Besides the historiographical part, Rudé's analysis of Robespierre in part 4 as "The Practitioner of Revolution"—some shortcomings aside—is the best section of the book. Specialists will differ with the discussion of some of the specific points but will agree with Rudé's conclusions about Robespierre as a major revolutionary leader.

WITH NORMAN HAMPSON'S WORK we come to a completely different approach to the study of Robespierre. Hampson had originally set out to write a biography. The more he studied the evidence, however, the more baffled he became. The sources gave support, he said, to conflicting interpretations. Describing himself as a "historical agnostic," he concluded that it was impossible to understand Robespierre if one approached him with impartiality. Instead, the author has tried to find room in his book for all the conflicting interpretations.

As a result, the book takes the unusual form of an exposition by a narrator—"I"—who acts as a leader of a discussion group, presenting the evidence, and allows three other persons—a civil servant from the ministry, a Communist party member, and a clergyman—to give their own viewpoints. (Hampson states that he shares their attitudes.) At a later point in the book a "J. S." appears who is nowhere identified but who seems to be the narrator.

Hampson has done much research on Robespierre and presents a carefully documented study of the man. The unusual technique he employs to present his findings is sometimes helpful, and at other times quite confusing. One of

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the reasons for the confusion is that he does not always make clear who is speaking. The discussions, furthermore, interrupt the story. The problem is compounded by the occasional attempts to arrive at value judgments of actions. The reader, as a result, does not get a clear picture of the historical developments. Hampson's reply to this criticism would probably be that he has achieved his objective, since the evidence allegedly leads to a variety of possible interpretations of Robespierre. More specifically, the author is obviously puzzled by the events leading to Thermidor. The book ends without any conclusion.

Robespierre "was a man of obvious importance who exercised considerable influence over some of the most significant events in modern history" (p. xi). Hampson thus gives great importance to the role of the leader in history. Not being a Marxist, he is generally cautious about the use of Marxist "class" terms and concepts of "class struggles." Though he does deal with social trends in his narrative, his emphasis is on Robespierre. Although generally sympathetic toward "this tormented and self-contradictory man" (p. x), he notes Robespierre's occasional viciousness, his ruthlessness, and his rage against suspected traitors. Hampson says that Robespierre's speeches were intended to persuade rather than to reveal himself (p. xi), that we shall never know what he really believed and how far his beliefs corresponded to what he said (p. 247). While it is true that Robespierre spoke to convince his audience, he was on many occasions so emotionally involved in his addresses that he revealed his personality and inevitably at least part of his innermost thoughts. How many times have critics of Maximilien denounced his addresses for being too much concerned with himself? Even Hampson occasionally notes that. If Robespierre's speeches and writings in general are read with a trained psychological eye, it should be possible to understand Robespierre better.

Hampson does reach some conclusions about Robespierre as a revolutionary leader. He writes that Robespierre developed a coherent view of events which probably accounted for his becoming the spokesman for the Committee of Public Safety and the Montagnards. Though Robespierre's ideas had evolved since 1789, he remained a champion of liberal, representative democracy. He hoped to end the revolutionary government and establish the utopia but was continuously frustrated. Hampson considers Robespierre generally as a shrewd political tactician. The author, however, does not know how to explain Robespierre's actions after Germinal. There is, of course, no overall description of Robespierre's personality, and Hampson avoids any attempt at psychoanalysis.

Hampson's treatment of Maximilien's life before the Revolution is generally acceptable. Some of his interpretations, however, are dubious. For example, his assertion that Maximilien did not feel frustrated on the eve of the Revolution (p. 24) is contradicted earlier (p. 11).[23] While the *mémoire* in the Dupond

[23] My interpretation of Robespierre's youth is different in important respects. See my article, *supra*, 21n. For further evidence of Maximilien's frustration, see L. Berthe, "Un Inédit de Robespierre. . . ," *Annales historiques de la Révolution française*, no. 216 (1974): 267, 272–73, 277.

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case appeared in 1789 (p. 29), probably in February or March, I have concluded that Robespierre had probably presented the main arguments orally to the court in December of the previous year. The correct dating of this material is important to the understanding of Maximilien's transformation into a revolutionary. The dating of Robespierre's dedication to Rousseau is a moot and perhaps insoluble point.

A number of miscellaneous comments on Hampson's analysis of Robespierre during the Revolution should be made. It is satisfying to find a scholar sympathetic to Robespierre who gives a correct view of Robespierre's role in the great *journées* of the Revolution (such as August 10 and May 31–June 2). Hampson correctly shows either the lack of adequate information on Robespierre's role or his caution or ambiguity during these events. Hampson is accurate when he minimizes Robespierre's interest in economic problems. Unlike the Marxist scholars discussed elsewhere in this review, Hampson recognizes that Robespierre's definition of property and the role of society were dominated by his prime concern with moral factors. The author maintains further that Robespierre never referred to the Ventôse decrees at all and, indeed, said nothing about social policy after he became a member of the Committee of Public Safety. He had more important things on his mind. Hampson's treatment of Robespierre's religious beliefs and policies is also correct. Robespierre was a deist with a belief in divine intervention in human affairs. In contrast, the author's interpretation of the period of the Convention is flawed by his not having read Alison Patrick's *The Men of the First French Republic: Political Alignments in the National Convention of 1792*. It may be that the book appeared too late for Hampson to use. Finally, a number of inaccuracies in dates and typographical errors are minor points of criticism. Despite its weaknesses, however, this is a book to be read and digested.

WOULD PSYCHOANALYSIS HELP US solve some of the mysteries concerning Robespierre?[24] Attempts to psychoanalyze Robespierre are not new. As early as 1924 Hans von Hentig, on the flimsiest of evidence, presented a confused, but hostile, psychological portrait of Robespierre.[25] Two recent works have tried to apply more careful approaches to the problem. Max Gallo's *Maximilien Robespierre, Histoire d'une solitude* (translated as *Robespierre the Incorruptible, A Psycho-Biography*) belongs to the genre of popular history and, we might also add, popular psychoanalysis. Gallo has studied the basic printed primary sources and some of the major secondary works. A clear-cut Marxist interpretation of the French Revolution pervades the book, which portrays Maximilien de Robespierre as a "bourgeois" revolutionary limited by his "class" origins. The English version, unfortunately, has many gaffes and serious errors in translation, some of which change the entire meaning of a concept.

[24] The psychoanalytic part of this article has benefited from the advice of Boris Heller, M.D., Fellow of the American Academy of Psychoanalysis.

[25] Hentig, *Robespierre, Studien zur Psycho-Pathologie des Machtriebes* (Stuttgart, 1924).

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The unreliability of the English translation requires that only the French original be read.

Gallo's psychoanalytic thesis is based on Maximilien's childhood: his birth four months after his parents' marriage and the instability of his father, who abandoned him when the mother died in her fifth childbirth. Maximilien, then only six, loved his mother, says Gallo, and felt guilty for his father's actions. In contrast to his irresponsible father he would be responsible and orderly and would adjust to society. He strove to be perfect. He had to find self-esteem, so he studied in school, tried to succeed as an advocate in Arras, did not marry but became head of a household shared with his sister Charlotte. He yearned for approval; but he wanted recognition for what he really was. Confidence and distrust in himself were combined. He knew he would fail. The quest for guilt and for recognition by others dominated him.

The Revolution gave him his opportunity. He did not have to please anyone. He was finally at war with the world, on the side of his fugitive father. He was certain of his own purity. His life became the Revolution; he had no ties to any individual. As early as 1789 what was to be his fundamental nature during the Revolution became apparent. He could only fight for the People. His superficial pride grew; he refused to compromise. The idealized People was the surrogate Father. Yet he was always alone, even at the peak of his popularity. He had to wait till 1794 for the punishment due him because of the sins he had committed in childhood against his father. He was fascinated with failure, defeat, and death in self-sacrifice that would be both supreme punishment and liberation, an absolution and a sanctification. Applause reassured him; any sign of hostility paralyzed him. He personalized debates; he was always the Savior and he knew it. Everyone else was suspect. He was not a man of action. Bouts of exhaustion and depression increased; these were flights from reality, says Gallo, but until near the end of his life he would recover and start the struggle anew. In the Committee of Public Safety his energy seemed to have increased tenfold. All his opponents were immoral and corrupt. Only he was pure. "*Ainsi la solitude est-elle au bout de cette psychose*" (French ed., p. 264). By Thermidor he found it increasingly difficult to overcome his psychological fatigue. He seemed to want the peace of death. The martyr's death he had been yearning for since 1789 finally came.

While Gallo at times shows some sympathy for Robespierre, he criticizes the limitations of the Artesian's views imposed by his so-called "bourgeois" status. For example, since he was "bourgeois," he was always ready to fight by himself but distrusted mass movements and insurrection (p. 253). For the same reason he saw issues only in terms of morality (pp. 89–90). The author thus at times uses Robespierre's "class" membership to explain his psychology.

Aside from this attempt to fuse Marx and psychoanalysis, what are we to say of Gallo's psychoanalysis of Robespierre? The hypothesis, first of all, does not explain Robespierre's numerous successes and leadership role during the Revolution. The explanation, if true, accounts for the failure in '94 but not

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Robespierre's positive role in the Revolution. What factors in Maximilien's upbringing and character development accounted for his successes? After all, Gallo refers to Robespierre as the leading spirit of the Committee of Public Safety, and notes his organizational gifts and popularity. This question is not really faced. Robespierre becomes a passive figure, not the Revolutionary leader he was many times.

Most important of all, what evidence does Gallo present for his psychoanalysis? Maximilien's emotional relationship with his fugitive father, which is the foundation of Gallo's hypothesis, may sound plausible, but it is not proven. Furthermore, that Robespierre had bouts of emotional and, hence, physical exhaustion is probably correct. But there is no historical or psychoanalytical evidence of any sort presented by the author that can explain them. The psychoanalysis is not based on any theoretical construct. The analysis is not Freudian or neo-Freudian—nor any other type. There is a good deal of description of Robespierre's psyche, but psychoanalysis explains why individuals act as they do. Gallo does not do this. His confusion of neurosis with psychosis indicates his unfamiliarity with psychiatric terminology.

It is interesting to note that Rudé, who has apparently only read the English translation of the volume and who obviously does not think much of "psychohistory," properly questions whether the theme of Maximilien's sense of his father's guilt can be verified. He describes the work as "well-meaning but naive."[26] Rudé's conclusion is close to the mark. Hampson (who read the French edition) asserts that Gallo's theory is persuasive but admits the lack of evidence. The evidence for Robespierre's loneliness is largely negative. Anyone, Hampson concludes, who started with assumptions different from Gallo's could arrive at different conclusions. Bruce Mazlish, a specialist in intellectual history and a protagonist of psychohistory, read the English version of Gallo after having written his own chapter on Robespierre in *The Revolutionary Ascetic*. He succinctly summarizes Gallo's thesis in a footnote and states that though the work is "written in a novelistic manner, it is psychoanalytically informed" (p. 235, 12n). As indicated above, I question the depth of Gallo's psychoanalytic understanding and insight.

Mazlish in *The Revolutionary Ascetic: Evolution of a Political Type* deals with Robespierre briefly.[27] Robespierre, however, is presented as an illustration in the emergence of the secular revolutionary ascetic, the prototype of whom is Lenin. Mazlish begins with an attempt to develop a theoretical construct, the ideal type of a revolutionary ascetic. Basing his theories on Weber and Freud, he finds, despite the varieties of modern revolutions, the existence of common personality traits in the revolutionary leaders: ascetic traits which are placed in the service of secular, modernizing revolution; and what he terms "displaced libido." Revolutionary asceticism becomes more and more prominent and functional as the twentieth century approaches. The ideal revolutionary ascetic denies "libidinal" ties to any individuals, practices self-discipline and

[26] Rudé, *Robespierre*, 234.
[27] A more general review of this book will appear in *AHR*'s April 1977 issue. (ed.)

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self-denial, and displaces his "narcissism" onto an abstraction with which he identifies. The functional advantage of such leadership is that it makes him appear superhuman, encouraging support from the faithful.

Basing his interpretation of Robespierre on a limited number of secondary works, Mazlish asserts that Maximilien was an example of the narcissistic revolutionary ascetic. But this took the form mainly of a simplicity of life which "initially helped him to lead his fellow French citizens" (p. 89). As a member of the government he grew paranoid, becoming unable to stabilize the Revolution, and was thereby led to his undoing. Unable, in addition, to develop a political party, he guaranteed his ultimate defeat. Robespierre was thus only "a pioneer . . . not a prototype of revolutionary asceticism" (p. 91).

The specialist in the French Revolution must regretfully state first that the factual information on Robespierre on which Mazlish's thesis is based is weak. Mazlish is not aware of any religious inspiration in Robespierre. On the contrary, the whole life of the Artesian from adolescence[28] through the Revolution clearly revealed his profound religious feelings.[29] While it is true that Robespierre showed paranoid tendencies during the Year II, he did not display more than other revolutionary leaders, even in the Committee of Public Safety. Since Mazlish follows Gérard Walter's treatment of Maximilien's youth, he repeats all of that writer's errors, for example, on the lightning-rod case, the Deteuf case, and his alleged exclusion from the juridical conferences in Arras in 1788.[30] Mazlish's criticism of Robespierre's inability to form a political party must be balanced by the consideration that there were no political parties in any modern sense during the Revolution. Political parties were impossible in the historical circumstances of the French Revolution.

The generally unsympathetic portrait of Robespierre that emerges takes no account of his periods of immense popularity as the champion of liberal democracy in the National Constituent Assembly or of his tremendous leadership abilities in the trial of the king and more especially as a member of the Committee of Public Safety. His speeches of 5 Nivôse and 17 Pluviôse of the Year II on the organization of revolutionary government contradict Mazlish's interpretation.

Most important, what can one say of Mazlish's psychoanalytic thesis concerning Robespierre? Does it help us to understand Robespierre better? Not really. Specialists knew before Mazlish wrote his book that Robespierre was "the Incorruptible," that the Revolution became his whole life, and that he probably had no sexual relations with women. Robespierre was much more complex than Mazlish imagines, and his role in the Revolution was similarly much more complex. Of course, it must be admitted that the author only considers Robespierre as a "pioneer" in the development of revolutionary asceticism.

[28] Shulim, "The Youthful Robespierre," 415–16.

[29] See, for example, his emotional response to Guadet in the debate at the Jacobins on March 26, 1792. *Oeuvres*, vol. 8, *Discours 3e Partie : Octobre 1791–Septembre 1792*, 233–36.

[30] Cf. Shulim, "The Youthful Robespierre."

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The fundamental psychoanalytical question is the validity of Mazlish's thesis in general. Since his theoretical construct is based on Freud, it should be noted that Karen Horney has questioned Freud's emphasis on the contention implicit in the libido theory that "all bodily sensations of a pleasurable nature, or strivings for them, are sexual in nature."[31] Mazlish does not even emphasize the sexual origins of the libido, applies the theory to mere personal relationships, and calls it "displaced libido." In any case, if we follow Horney's theory rather than Freudian theory, every psychic phenomenon is determined in multiple ways. She asserts that the libido theory is an oversimplification. Man, according to her, is ruled not by the pleasure principle alone but by two guiding principles: safety and satisfaction.[32] In other words, if one does not confine onself to orthodox Freudian psychoanalytic theory, Mazlish's speculations become highly questionable.

If we disregard the psychoanalytic foundations of the book and examine its value for an understanding of modern revolutionaries, it can be considered an interesting hypothesis which helps partly to understand some events better. The best chapter is the one on Lenin. The author himself, to his credit as a scholar, recognizes the limitations of his theory and admits that there are other ways of approaching the psychoanalysis of revolutionary leaders.

In conclusion, the application of psychoanalysis to history should be attempted by specialists in the particular historical epoch. It should, furthermore, be carefully grounded in the latest psychoanalytic approaches—going beyond Freud—and it should at all times stress the speculative nature of the attempt.

DESPITE THE RECENT SPATE of writings on Robespierre, an up-to-date biography of that revolutionary leader still remains to be written. The ideal biography, in this reviewer's opinion, would be one by a historian familiar with the original sources on Robespierre and his times, a specialist in the *ancien régime* and the Revolution, who is not bound by any preconceived notions, whether Marxist or otherwise, who would be willing to let the facts lead him to conclusions, even if—where there are lacunae or contradictory evidence—some of the conclusions must be tentative, and who would also be able and willing to apply the latest psychoanalytic techniques—conjectural though the results may be—to shed further light on the life of Robespierre and on his role as a leader of the Revolution.

[31] Karen Horney, *New Ways in Psychoanalysis* (New York, 1939), 50.
[32] *Ibid.*, 59, 63, 64, 68, 73.

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# United States Relations with Latin America: Neighborliness and Exploitation

A Review Article by DAVID M. PLETCHER

GENE M. BRACK. *Mexico Views Manifest Destiny, 1821–1846: An Essay on the Origins of the Mexican War*. Albuquerque: University of New Mexico Press. 1975. Pp. vii, 194. $12.00.

DANA G. MUNRO. *The United States and the Caribbean Republics, 1921–1933*. Princeton: Princeton University Press. 1974. Pp. x, 394. $17.50.

DICK STEWARD. *Trade and Hemisphere: The Good Neighbor Policy and Reciprocal Trade*. Columbia: University of Missouri Press. 1975. Pp. ix, 307. $12.50.

STANLEY E. HILTON. *Brazil and the Great Powers, 1930–1939: The Politics of Trade Rivalry*. (Institute of Latin American Studies, Latin American Monographs, No. 38). Foreword by JOSÉ HONÓRIO RODRIGUES. Austin: University of Texas Press. 1975. Pp. xxii, 304. $9.95.

SURENDRA BHANA. *The United States and the Development of the Puerto Rico Status Question, 1936–1968*. Lawrence: University Press of Kansas. 1975. Pp. x, 290. $11.00.

LUIS VALLENILLA. *Oil: The Making of a New Economic Order. Venezuelan Oil and OPEC*. New York: McGraw-Hill Book Company. 1975. Pp. xviii, 302. $16.95.

F. PARKINSON. *Latin America, the Cold War, & the World Powers, 1945–1973, A Study in Diplomatic History*. (Sage Library of Social Research, Vol. 9). Beverly Hills, Calif.: Sage Publications. 1974. Pp. 288. Cloth $11.00, paper $7.00.

GORDON CONNELL-SMITH. *The United States and Latin America, An Historical Analysis of Inter-American Relations*. New York: John Wiley and Sons (A Halstead Press Book). [1974]. Pp. xviii, 302. $16.75.

LESTER D. LANGLEY. *Struggle for the American Mediterranean: United States-European Rivalry in the Gulf-Caribbean, 1776–1904*. Athens: University of Georgia Press. 1976. Pp. xii, 226. $10.00.

IF PUBLIC CONTROVERSY ENCOURAGES SCHOLARSHIP, the field of United States–Latin American relations is surely so well cultivated as to produce a dependable harvest of first-rate books. It is probably no coincidence that the decade following the Spanish-American War, which witnessed the "great



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debate" over imperialism in the Philippines, the occupation of Cuba, and the taking of Panama, also brought forth the first generation of historians easily identifiable as specialists in some part of hemispheric relations—John H. Latané, James Morton Callahan, Ira D. Travis, Jesse S. Reeves, W. F. Reddaway, and others even more obscure today. Many later historians were affected directly by events of their own day. For example, the problems of relations with revolutionary Mexico after 1910 stimulated George L. Rives and Justin Smith to publish works on American-Mexican relations in the 1820s and 1840s; and J. Fred Rippy and Leland H. Jenks produced "Studies in Imperialism" on Colombia and Cuba respectively to answer a cry in the 1920s for more information on "dollar diplomacy."[1] During the Depression, the Good Neighbor period, and World War II, increased public interest in Latin America was reflected in broader and deeper study of hemispheric relations in the past. By 1945 the field was so well established that the postwar shift of public attention to Russia, China, Vietnam, and the Middle East did not cut off the flow of dissertations and published monographs about the United States and Latin America.

Has quality matched quantity of output? No litmus test or computer exists to answer this all-important question; yet there are a few disturbing indications. For one thing, the "classic" synthesis of the subject—and also the only one available in a paperback edition from a major commercial publisher—is *The Latin American Policy of the United States* by Samuel Flagg Bemis, published in 1943 and never revised.[2] While we have reasonably up-to-date surveys of United States relations with Mexico, Cuba, Argentina, and Chile,[3] the most recent general study in English of relations with Brazil was published in 1932, with Colombia in 1935, and with Haiti in 1940 and 1941.[4] Comprehensive, systematic books in English on United States relations with Venezuela, Ecuador, Bolivia, Paraguay, and Uruguay do not exist. The standard works on hemispheric relations in the Independence period and on the evolution of the Monroe Doctrine date from 1941.[5]

If it were possible briefly to describe the next level of monographs, those dealing with limited periods, episodes, or special types of relations (e.g. economic or military) with one or several Latin American nations, we would doubtless find a few subjects thoroughly covered, a good many more "opened

[1] George Lockhart Rives, *The United States and Mexico, 1821–1848* (2 vols.: New York, 1913); Justin H. Smith, *The War with Mexico* (2 vols.: New York, 1919); J. Fred Rippy, *The Capitalists and Colombia* (New York, 1931); Leland H. Jenks, *Our Cuban Colony, A Study in Sugar* (New York, 1928).

[2] Samuel Flagg Bemis, *The Latin American Policy of the United States* (New Haven, 1943).

[3] Karl M. Schmitt, *Mexico and the United States, 1821–1973: Conflict and Coexistence* (New York, 1974); Lester D. Langley, *The Cuban Policy of the United States, A Brief History* (New York, 1968); Harold F. Peterson, *Argentina and the United States, 1810–1960* (New York, 1964); Fredrick B. Pike, *Chile and the United States, 1880–1962: The Emergence of Chile's Social Crisis and the Challenge to United States Diplomacy* (Notre Dame, 1963).

[4] Lawrence F. Hill, *Diplomatic Relations between the United States and Brazil* (Durham, N. C., 1932); E. Taylor Parks, *Columbia and the United States, 1765–1934* (Durham, N. C., 1935); Ludwell Lee Montague, *Haiti and the United States, 1714–1938* (Durham, N. C., 1940); Rayford W. Logan, *The Diplomatic Relations of the United States with Haiti, 1776–1891* (Chapel Hill, 1941).

[5] Arthur Preston Whitaker, *The United States and the Independence of Latin America, 1800–1830* (Baltimore, 1941); Dexter Perkins, *Hands Off: A History of the Monroe Doctrine* (Boston, 1941). In some of the later editions the title was shortened to *A History of the Monroe Doctrine*.

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up" for further consideration, and a third group of undetermined size either ignored or left to outdated scholarship. Why does the field of inter-American relations present such a patchy, weedy appearance, fresh green growth and mature plants alternating with drying stalks, husks, and bare ground? Some of the principal problems confronting historians of this field may be summarized under three headings: conceptualization, balance, and source materials.

The principal conceptual problem facing students of inter-American relations is that of "the one and the many." Since the early nineteenth century the United States has claimed to have a "Latin American policy," whether under the caption of Monroe Doctrine, pan-Americanism, the Good Neighbor, or the Alliance for Progress; but it has maintained relations with individual Latin American nations, adjusting its general policy to each country. James K. Polk would not have applied the Monroe Doctrine similarly to Argentina and Mexico; Richard M. Nixon would never have approved similar aid programs for Brazil's military government and Salvador Allende's Chile. Though historians may easily trace relations with an individual Latin American nation, when they try to deal with a larger area such as the Caribbean or with all Latin America, they find themselves skipping from general to particular, between this country and that, to their own confusion and that of the reader. And while political relations may be contained within national borders, this is less often true of business expansion. United Fruit operated in many countries under many different "arrangements"; so did oil and mining companies, manufacturers like Firestone and Ford, and merchandisers like Sears.

But even where only one Latin American nation at a time is involved, another conceptual problem troubles the historian. During the nineteenth century European (especially British) diplomats and businessmen dominated most of South America. Only in Mexico and Cuba can one find long-lasting, overarching American diplomatic problems and steadily expanding American economic interests—the basic components of writings on international relations—from 1820 to the end of the century. Elsewhere there exist few threads on which to string apparently unrelated events, except for the never-ending claims litigation, in which most readers' interest is limited. The Spanish-American War and the subsequent American interventions and protectorates created a continuous "history of relations" for the rest of the Caribbean. Not until the 1930s, however, did American relations with Argentina or Brazil reach this stage of historiography. (Thanks to the War of the Pacific, James G. Blaine, and the Tacna-Arica question, United States–Chilean relations exhibit at least a continuity of diplomatic blunders and coolness after 1880.) American relations with Ecuador, Bolivia, Paraguay, and Uruguay never reached this stage, a fact which probably explains why no one has bothered to publish a survey account of them.

A second set of problems confronting the historian of United States–Latin American relations is that of maintaining a balanced view through long exposure to highly emotional, exaggerated rhetoric on each side. Fortunately most Americans were seldom as vituperative as those Texans of the 1860s who

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characterized Mexicans as "a mongrel race of bloody minded brutes" or "scavengers and vagabonds, downright barbarians but a single remove above the Digger Indians, hanging like vermin on the skirts of civilization."[6] And few Latin Americans matched the ex-president of Guatemala who described the United States as a shark—"this giant of the sea, one-time criminal, bandit, master delinquent, and model pirate—today converted to the religion of the Law . . . [who would] be willing to control you, to help you, to protect you, to have you . . . every day nearer, until one day you become encrusted as an oily granule in his grotesque skin."[7] Latin Americans, while usually less abusive than this, often resorted to direct attack on United States motives and methods, while writers in the United States were more likely to offend Latin Americans by condescending pity, either assuming that they were incapable of matching American achievements or that it was the Christian duty of the United States to help them do so. Even the sympathetic Franklin D. Roosevelt sometimes patronized Latin Americans, as when he told newspapermen in 1940: "Give them a share. They think they are just as good as we are and many of them are."[8]

The historian, seeking to discover "public opinion" in speeches, editorials, and letters has commonly encountered wildly contradictory charges and counter-charges of inherent inferiority, greed, corruption, and hypocrisy. Much of the rhetoric arises from conflicting sets of values: material versus nonmaterial, Roman Catholic versus Protestant, English versus Spanish, common law versus Roman law. But if he wholly identifies any of these with either side—if, for example, he accepts Latin American attacks on United States materialism without a glance at *latifundismo* or the *mordida*—he may forfeit all hope of objectivity. Instead, he must laboriously examine writings on both sides to determine their applicability to the case at question in the light of verifiable evidence.

The problem of balance is made all the more difficult in that the bulk of works dealing with United States–Latin American relations have hitherto been written by Americans. Latin American historians have made some important contributions to the historiography of their own countries and, considerably less often, to that of their relations with the United States. American historians with access to large research libraries routinely make use of these Latin American volumes, but small printings and infrequent translations shut them off from any but specialists. (A welcome if rare exception is Nettie Lee Benson's excellent translation of the section of Daniel Cosío Villegas' monumental *Historia moderna de México* most pertinent to American-Mexican relations.)[9] A worthy project for some foundation would be a series of translations of Latin American monographs on phases of hemispheric

[6] Quoted in Michael G. Webster, "Texan Manifest Destiny and the Mexican Border Conflict, 1865–1880" (Ph.D. dissertation, Indiana University, 1972), 65, 88.

[7] Juan José Arévalo, *The Shark and the Sardines*, tr. June Cobb and Raúl Osegueda (New York, 1961), 34.

[8] Press conference, January 12, 1940, quoted in Bryce Wood, *The Making of the Good Neighbor Policy* (New York, 1961), p. 359.

[9] Daniel Cosío Villegas, *The United States Versus Porfirio Díaz*, tr. Nettie Lee Benson (Lincoln, 1963).

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relations, each edited with a commentary by a United States historian in a related field.

Closely related to the imbalance of attitudes and writings is the problem of gaining access to primary sources dealing with inter-American relations. The historian studying the last quarter-century of the field—for example, the United States' break with Fidel Castro or the Alliance for Progress or the 1965 Dominican intervention—must usually rely on printed sources in government documents or newspapers (unless he finds a windfall of private papers), while he waits for the United States National Archives, in the fullness of time, to gather, collate, check, declassify, and open unpublished records.

Latin American materials are almost invariably less accessible and more disorganized than those in the National Archives; and only a few countries, especially Mexico and Brazil, have made any systematic effort to aid scholars, whether their own or those from abroad. The same discrepancy applies to private papers. American collections are far from uniform—witness the contrast between the mass of William H. Seward papers at the University of Rochester and the few scraps of James G. Blaine's manuscripts scattered about in several places. But even in Mexico and Brazil few private papers have been turned over to public archives, and the voluminous, well-filed collection of letters and memos is exceptional indeed.

A third category of primary sources, often just as important for a reliable knowledge of United States–Latin American relations as public documents and private correspondence is that of business archives. In this area historians dealing with the domestic operations of American companies have been more successful than those dealing with foreign operations, but even their success has often resulted from diplomacy no less patient or skillful than that between veteran ambassadors and foreign ministers. While the archives of United Fruit or Anaconda or ASARCO are perhaps not as jealously guarded as those of the Kremlin, the fact remains that much history-writing on American business operations in Latin America has been based largely on published company reports and handouts or on editorials and pamphlets from the Latin American opposition—and the New York *Times*.

With these preliminaries, let us examine a group of recent books about United States–Latin American relations, including both monographs and surveys. Like most random samplings, this is a mixed bag, but it may tell us a little about the condition of the field.

THE EARLIEST WRITING, in point of subject, is Gene M. Brack's description and analysis of Mexican newspaper writings from Independence to the war with the United States. Its dramatic title is a bit oversimplified, for in these writings the Mexican press (not Mexico) is viewing not only manifest destiny but also American democracy, slavery, and treatment of Indians. Brack is less concerned than Charles Hale with ideological rapport between Mexican

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liberals and American liberal democracy,[10] but he agrees with Hale that the early Mexican view was ambivalent and hazy: "To most Mexicans Americans remained a strange, alien people, seen in dim focus from a great distance" (p. 50). This condition seems to have prevailed among the public well into the 1830s, although by the middle of the decade newspapers were loudly denouncing "the colossus of the north." Clearly the Texas question was largely responsible for this boiling up of condemnatory journalism, but Brack tries—not wholly convincingly—to blame a considerable part of the anger on revulsion against slavery. He admits that some early writers paid more attention to American treatment of the Indians—a natural enough reaction in view of Mexico's own large Indian population. When he gets to the reporting of American atrocities against Indians and blacks in the 1840s, one may well wonder whether this reporting was a cause of Yankeephobia, as he seems to think, or both cause and effect.

Brack's ransacking of news stories and editorials is a great service to the American student, but the latter may want to supplement the book with an equally interesting overview of Mexican journalism during and after the war, *La guerra del 47 y la opinión pública,* published at the same time as Brack's monograph by an American-trained Mexican, Jesús Velasco Márquez.[11] The two authors examined many of the same publications and treat of similar themes. Naturally they sometimes disagree. For example, after examining issues of *El siglo XIX* for late March 1845, Brack finds this important newspaper demanding war, while Velasco Márquez cites a call for recognizing Texan independence in the issue for April 9. Among the main points of Velasco Márquez's thesis is the argument that those of the press who desired war did so mainly for logical reasons, not patriotic fervor, and that the long-range effects of the war were mostly favorable to Mexico, especially in inducing self-examination and reform.

Brack's ambitious subtitle, *An Essay on the Origins of the Mexican War,* indicates an unfortunate split in this short monograph between press opinion and larger complexities. In the 1820s, for example, he offers a too-simple account of Joel Poinsett's mission in which he fails to explain the overwhelming Mexican animus against the American minister and virtually ignores Henry George Ward and British influence. Brack declares that "one of Poinsett's objectives" (by instruction, presumably) was "to negotiate a treaty of limits, and in doing so to reassert his country's claim to Texas" (p. 63)—which is misleading, to say the least. In the late 1830s he relies heavily on the bitterly Yankeephobe view of *El mosquito mexicano* toward William Parrott, an American claimant and later special agent. This is fair enough in a study of press opinions but not in an analysis of origins. In the last chapter Brack makes use of hitherto unavailable archival material to re-emphasize the essential peaceableness of the José Herrera administration. By then he has almost lost sight of the press.

[10] Charles A. Hale, *Mexican Liberalism in the Age of Mora, 1821–1853* (New Haven, 1968), ch. 6.
[11] Jesús Velasco Márquez, *La guerra del 47 y la opinión pública (1845–1848)* (Mexico City, 1975).

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Although it is well known that most inhabitants of Mexico City were highly literate in the 1830s and 1840s, this is no guarantee that they read the newspapers Brack and Velasco Márquez have examined. Nevertheless, these careful, penetrating studies bring us as close to Mexican public opinion as we are likely to get. Another recent monograph by John H. Schroeder, *Mr. Polk's War*, similarly sets forth antiwar opinion in the United States, and, of course, the older works of Albert K. Weinberg and Frederick Merk have done the same for the expansionists.[12] We now have a good idea of how both sides viewed the war and also, largely from the American viewpoint, many detailed accounts of its diplomatic and military history. The widest gap in Mexican War historiography that remains to be filled by up-to-date scholarship is Mexican domestic history during the war: political intrigue, financial expedients, party battles, and their relationship to the bitter Church-State showdown.

Skipping over the next eighty years, let us examine Dana G. Munro's account of the United States' policies toward its Caribbean protectorates during the 1920s and early 1930s—a sequel to his *Intervention and Dollar Diplomacy in the Caribbean, 1900–1921*.[13] As in the earlier book, he relies mainly on official records and tries to explain State Department actions with heavy emphasis on administrative problems, financial agreements, and official contacts with the principal leaders of Cuba, the Dominican Republic, Haiti, and Nicaragua. (The rest of Central America gets short shrift, and he explicitly rules out Panama as a special case.)

The new book does not quite live up to *Intervention and Dollar Diplomacy*, perhaps because Munro served in the State Department throughout the period it covers, including assignments as legation secretary in Panama and Nicaragua, chief of the Latin American Division, and finally (1930–32), minister to Haiti. He makes a valiant effort to be impartial, and his personal experience gives added depth and detail to discussions of administrative problems, but this experience needs to be supplemented with private papers, memoirs, and secondary works, which are cited less frequently than in the earlier volume. (Comparison is difficult, since neither book contains a bibliography.) Probably also his emphasis on political, legal, fiscal, and military matters was more appropriate to the earlier period, when the United States was mainly concerned with creating the protectorates, than to the 1920s and 1930s, when it was trying to reduce its involvement.

At his best, Munro offers a clear, balanced view of difficult administrative choices and reasoning. A good example is his account of how General Enoch H. Crowder attempted to prod Cuban electoral machinery into effective action. Staying on reluctantly as adviser in 1921, Crowder at first declined a more seemly appointment as minister to Cuba in order not to forfeit his Army

[12] John H. Schroeder, *Mr. Polk's War: American Opposition and Dissent, 1846–1848* (Madison, 1973). Albert K. Weinberg, *Manifest Destiny, A Study of Nationalist Expansionism in American History* (Baltimore, 1935). Frederick Merk, *Manifest Destiny and Mission in American History, A Reinterpretation*, with the collaboration of Lois Bannister Merk (New York, 1963).

[13] Dana G. Munro, *Intervention and Dollar Diplomacy in the Caribbean, 1900–1921* (Princeton, 1964).

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pension (a wholly plausible administrative technicality). These are the key sentences of Munro's analysis:

> The election [of March 1921] showed how difficult it is to impose democracy by pressure from outside. . . . The alternatives were to hold aloof and risk a civil war in Cuba, or to compel the holding of a new election, presumably under complete American control. . . . It is true that his [Crowder's] activities during the first years of [President Alfredo] Zayas' administration involved a great deal of interference in Cuba's internal affairs. His purpose, however, was not to establish permanent American control but to correct conditions which would expose the republic to foreign interference or eventual armed intervention. He was genuinely interested in the effort to develop stable republican government in Cuba and he thought that this effort could not succeed unless the political evils and the gross corruption which had brought the country to the verge of disaster were eliminated. He thought that the United States had a duty, under the Platt amendment, to insist on reforms and to resort to compulsion if its advice were disregarded (pp. 21–23).

Here Munro presents in a nutshell the central dilemma of the United States protectorate policy. He is not always so straightforward. For example, in explaining how and why the State Department hoped to withdraw from Nicaragua in 1924, he admits that the ensuing Liberal government "would probably make trouble for the customs receivership and the New York bankers, but I do not remember that we were particularly concerned about this aspect of the situation. I imagine that we assumed that the American government would still have enough influence to see that any new Nicaraguan administration honored its contractual obligations" (p. 162). This dry remark is Munro's closest approach to sarcasm, and it evades the crucial issue. How could the United States government withdraw and still exert "influence" without intervening?

The limitations of Munro's background are most obvious in his omissions. While giving full accounts of loans and negotiations with financiers, he has little to say about other American economic interests. United Fruit is almost ignored. (To be sure, it was most powerful in Central American countries where the United States did not have formal protectorates.) The Cuban sugar industry gets several brief references totaling about a page of text. The Haitian Corporation of America, a kind of holding company which controlled most of the country's sugar industry, railroads, and utilities, receives a little more attention, mainly as a subsidiary of the National City Bank of New York. A far more serious blind spot is Munro's striking inattention to American treatment of the common people and to problems of race relations. He points out the differences between the Dominican Republic and Haiti that, the State Department felt, justified withdrawing the marines from the former in the mid-1920s but not the latter—the relatively homogeneous, Spanish-speaking populace and the organized political parties. Also he admits that the department was unhappy with the increasingly transparent fiction that the Haitian occupation government represented the people. Nevertheless, the people remain shadows, obscured by governmental machinery and financial arrangements.

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It is interesting to compare Munro's chapters on Haiti with a recent excellent monograph by Hans Schmidt.[14] At the outset Schmidt poses a conflict between a "technical, materialistic approach to reform" (p. 15) and an ingrained, traditional social order, complicated by race prejudice originally imported by United States marine officers and their wives. He then proceeds to show how the paternalistic Americans, sympathetic with the lower-class blacks who "knew their place," alienated the upper-class mulattoes, the natural ruling elite, and tried to erect an elaborate superstructure of modern improvements on the weakened foundation of a divided, partly hostile people who neither understood nor wanted the improvements. Munro amply describes the superstructure. If he recognized the foundation weaknesses while he lived in Haiti, he has not paid them much attention here.

This is not to say that Schmidt is consistently superior to Munro; they frequently reach similar conclusions, and sometimes Munro's background and experience give him advantages over the younger researcher. For example, Munro offers a fuller account of American efforts to reform corrupt Haitian courts—yes, even a better description of press censorship by the occupation government! Where Schmidt merely criticizes the pre-1922 military regime for lack of educational reform, Munro explains that President Sudre Dartiguenave rejected all advice—everything, indeed, except money for the schools. The two versions of the crucial loan negotiations beginning in 1922 typify the different approaches of the two writers. Schmidt gives the outlines of the negotiations, Munro much more of the intricate details, but both conclude that National City Bank's profits therefrom (and from its other Haitian operations), while high, were not exorbitant. Both admit that after servicing the resulting public debt, the Haitian government had little left over from its revenues to finance public works, but Schmidt emphasizes this fact more than Munro as a shortcoming of the American protectorate. What was the connection between the loan negotiations of 1922 and the contemporaneous American decision to continue the occupation (with less show of military force)? Munro implies that Haitian lack of preparation for democracy inspired the decision, which, in turn, brought the loan negotiations. Schmidt states emphatically: "The 1922 loan committed the United States government to long-term supervision of Haitian finances, since it would be called upon to protect the interests of American bondholders until the loan was fully paid" (p. 133). The reader may choose between the chicken and the egg.

Clearly, where the Haitian occupation is concerned, we need both accounts: Munro's formal, "official," and a bit bloodless; Schmidt's *simpático,* alive to social problems, and more disposed to criticize than to understand the American dilemma. In the other protectorates too, Munro will need to be supplemented by books of such authors as Robert F. Smith (on Cuba) and Neill Macaulay (on Nicaragua), as well as by other monographs yet unwritten.[15]

[14] Hans Schmidt, *The United States Occupation of Haiti, 1915–1934* (New Brunswick, 1971).

[15] Robert F. Smith, *The United States and Cuba, Business and Diplomacy, 1917–1960* (New York, 1960); Neill Macaulay, *The Sandino Affair* (Chicago, 1967).

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SEVERAL VOLUMES IN THIS COLLECTION are focused on the Good Neighbor period of United States–Latin American relations. Appearing at roughly ten-year intervals, three volumes by Edward O. Guerrant, Bryce Wood, and David Green attempted to survey most or all of the period and the policies associated with Franklin D. Roosevelt's famous phrase.[16] Guerrant offered mainly a chronological account of events; and Wood, relying on State Department documents and the Roosevelt papers, examined the origins of the Good Neighbor policy, its general formulation, and its principal tests in bilateral relations. Green, the only one of the three to cover the years of World War II in detail, had access to a wider range of private papers than the other two and put forth a much-criticized revisionist interpretation, trying to prove that Good Neighbor rhetoric masked a United States effort to "contain" Latin America, especially its reformist impulses. The period is open to a new synthesis, for which monographs such as the ones under review will make important contributions.

In one of them Dick Steward has drawn up a critical but, on the whole, reasonable assessment of the Reciprocal Trade Agreements Act of June 12, 1934, and of the eleven reciprocity agreements negotiated under it with Latin American nations during the next six years—a subject given little attention by Guerrant, Wood, and Green. Steward sees the reciprocal-trade program in Latin America as an effort to reconcile the sincere internationalist free-trade ideals of Cordell Hull and his supporters with the efforts of economic nationalists to expand American markets and further recovery from the Depression. He regards the agreements as successful only in the sense that during the late 1930s trade increased faster with agreement countries than with non-agreement countries. Most of the major Latin American countries rejected them; they aroused much Latin American resentment as instruments of American imperialism (which Hull did not intend); they soon gave way to the Export-Import Bank as the major force in hemispheric economic relations; and they were overwhelmed by wartime complexities. Steward regards Hull's ideals and thus, by implication, the whole reciprocity program as anachronistic. Hull, he says, "never realized that the principles of self-determination, mutual gain, and equality of nations were unobtainable when confronted by the vast economic powers of the American business system" (p. 281).

This forceful but defensible interpretation falls mercifully short of the New Left propensity to see a unified, irresistible, yet covert design for world domination behind every economic policy or measure of the United States during the 1930s and 1940s. Still, Steward sometimes resembles New Left writers in his inability to avoid backhanded jabs at American businessmen and their allies in the government. In two cases he gives undue prominence to damaging phrases quoted out of context by putting them in chapter titles. (In one case the author of the phrase is not even identified.) When Hull and his staff analyze Latin American economies, pointing out the vulnerability of one-

[16] Edward O. Guerrant, *Roosevelt's Good Neighbor Policy* (Albuquerque, 1950); David Green, *The Containment of Latin America* (Chicago, 1971).

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crop systems, this is called "taking advantage of Latin America's lack of export diversification" (p. 62). On slight evidence Steward pronounces the resignation of Gerardo Machado in Cuba and the establishment of a pro-American government "a *quid pro quo* for a revision of the 1902 Reciprocity Treaty" (p. 94). (Green, whom he does not cite, makes much the same point on even less evidence.) When Hull answers business criticisms of reciprocity with a memorandum setting forth its advantages to American trade, Steward censures him for showing "little or no concern for either uplifting the masses of Latin America through the use of U.S. funds and investments or building viable economies" (p. 140).

These gratuitous or dubious slurs are unnecessary, for Steward has offered much real evidence of the Roosevelt administration's ambivalence and contradictions. Unfortunately, they are not the only flaws in the book. Its organization is sometimes loose and its writing often muddy. Also, instead of devoting a whole chapter to Hull's controversy with the agricultural nationalist George Peek, Steward might well have used much of the space for brief summaries of the pre-1933 background of reciprocity and unconditional most-favored-nation provisions. Finally, while he draws from a wide assortment of government documents, his bibliography omits some of the now-accessible private papers of many policy-makers concerned with the reciprocal-trade program (such as Herbert Feis, Henry Morgenthau, Hugh Gibson, and Phillip Jessup) and a few useful business journals concerned with foreign trade.

Steward's moderately revisionist interpretation of Roosevelt's economic policies for the hemisphere is even more convincingly set forth in Stanley E. Hilton's study of Brazilian-American-European relations during the 1930s—the only book in this assortment based on truly multiarchival research. The United States-Brazilian reciprocal-trade agreement was undoubtedly the most important signed under the act of 1934. Shrewdly playing the United States off against Nazi Germany, which also wanted to expand its trade with Latin America, the Brazilian dictator Getúlio Vargas counteracted the effect of American reciprocity by negotiating an informal "gentleman's agreement" with Germany and approving use of ASKI (blocked) marks to pay for Brazilian exports. Not until after 1939, when the British blockade shut off German shipments of arms and other manufactures, did Brazil move definitively toward full hemispheric partnership, economic and political. What Hull regarded as a "crusade for economic sanity" rested in considerable part on substituting multilateral for bilateral trading practices; in Brazil this did not happen. Hilton concludes that on this basis "the example of Brazil points up the need for a revision of prevailing notions about the effectiveness of Good Neighbor diplomacy. For in Brazil, a country Hull regarded as crucial to his plans, American policy met with a resounding rebuff" (p. 226).

Hilton approaches this dismal conclusion by way of comprehensive, penetrating accounts of each participant's actions, based on thorough examination of sources in Brazil, the United States, Germany, and Britain, whose com-

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mercial influence, though fading, was still an international factor to be considered. Among Hilton's most interesting and significant contributions is a keen analysis of the Brazilian situation and outlook. He gives Vargas and his economic advisers credit for greater breadth and sophistication than most observers have realized and shows clearly that they were committed to long-range industrialization of their country from the outset, although held back by many handicaps of underdevelopment—for example, chaotic and unreliable statistics about every aspect of the Brazilian economy.

In setting forth German and American policies, Hilton is covering more familiar ground, but he adds much to existing accounts. His treatment of German-Brazilian relations is certainly the best in English, far surpassing the brief summary in Alton Frye's general study of the Nazi activity in the Western hemisphere.[17] Hilton's account of the United States-Brazilian trade agreement supplements Steward's nicely. Hilton has more to say about Oswaldo Aranha, the Yankeephile Brazilian ambassador and a key figure throughout, as well as about Brazilian reactions; Steward presents a fuller description of American reactions. Careful students of the final intricate negotiations (which involved both countries' relations with Germany too) will want to examine and collate both versions; they are essentially similar but present some differences of detail. Not surprisingly, Hilton has far more to say than Steward about the later stages of German-American rivalry for Brazilian trade. For example, where Steward barely mentions Hull's refusal to approve retaliation against Germany and Brazil and Aranha's disappointing mission to the United States (as Brazilian foreign minister) in early 1939, Hilton devotes several pages to each. Indeed, he regards the former as a most revealing indication that the American program of conciliation and persuasion had failed. "The Good Neighbor Policy was probably the most potent weapon in Brazil's diplomatic arsenal," he says, "as Brazilian officials masterfully exploited Washington's unwillingness to adopt sanctions against Brazil . . . and its willingness to be satisfied with rhetoric in pursuing its commercial aims" (p. 227).

Hilton's account of Brazilian-American relations overlaps in time span another excellent recent study of the subject by Frank D. McCann, Jr., who starts with the foundation of Vargas's Estado Nôvo in 1937 and runs through World War II.[18] In a foreword to Hilton's book, the Brazilian historian José Honório Rodrigues mistakenly dismisses McCann's account of the trade dispute as "based essentially on American documents and . . . hence unidimensional" (p. x). To the contrary McCann consulted the Aranha papers and documents of the Brazilian Foreign Affairs archives, and elsewhere he has also made extensive use of the Vargas papers, Brazilian military archives, newspapers, and interviews. (Both authors, of course, do full justice to United States sources.) It is true that McCann does not attempt to discuss Brazilian-

[17] Alton Frye, *Nazi Germany and the American Hemisphere, 1933–1941*. Yale Historical Publications, Miscellany 86 (New Haven, 1967).

[18] Frank D. McCann, Jr. *The Brazilian-American Alliance, 1937–1945* (Princeton, 1973).

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German relations in detail and that, in general, he emphasizes economic problems less and social and cultural relations more than Hilton. He devotes even greater space than Hilton to internal Brazilian history and gives a little fuller and clearer picture of the years in which the two books overlap—for example, concerning the Lima Conference of 1938 and the Aranha mission of 1939. Even in commercial matters, McCann explains somewhat more distinctly than Hilton how the German ASKI marks functioned in international trade. Nevertheless, the two books are essentially complementary, not competitive, and should be read (and owned) together. Thanks to Hilton and McCann, we now have a more authoritative record of United States-Brazilian relations than of any other aspects of the Good Neighbor period, including World War II.

SOME PURISTS MAY OBJECT to the inclusion of Surendra Bhana's account of the Puerto Rican status question in a review article devoted to United States relations with Latin America. In recent years Puerto Rican exponents of statehood or commonwealth and ultranationalists elsewhere in the Caribbean have tended to look at each other and mutter, "There but for the grace of God go I." It is becoming increasingly clear that the Caribbean protectorates of the early twentieth century and the Puerto Rican colony-commonwealth represent two alternative lines of development for United States influence in Latin America. It is time someone essayed a comparative study, drawing freely on political, legal, economic, social, and cultural materials.

Surendra Bhana has had no such intention. Although one might expect some sort of special insight into colonialism from an Indian lecturer on history at the University of Durban-Westville (South Africa), he has produced an orthodox, carefully researched but narrowly limited monograph on the parallel development of Luis Muñoz Marín's Partido Popular Democrático (PPD) and of American and Puerto Rican support for a compromise solution to the Puerto Rican status problem—the Commonwealth. This is a study of political maneuvering and argumentation which stays close to Puerto Rican newspapers and pamphlets and American executive and congressional documents, well illuminated by material from the private papers of United States political leaders. Bhana does not make personalities come alive; clearly Muñoz Marín owed much of his success to his charisma, but one would never know it from this description of him. As a result, it is a little hard to understand why the American Congress so easily accepted the commonwealth compromise in the climactic debate of 1950, although Bhana gives a full account of the preliminary publicity campaign and the congressional debate. Needless to say, economic and social conditions on the island fall mostly outside the purview of this study, although Bhana gives the principal administrative specifications of "Operation Bootstrap" in a short chapter near the end.

Bhana's book thus considerably resembles those of Munro and shares their strengths and weaknesses. Fortunately other works on Puerto Rico fill some gaps. Gordon K. Lewis has written a comprehensive, sensitive survey of the

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island in the 1940s and and 1950s, emphasizing society and culture.[19] Thomas Mathews soberly describes New Deal policies during the blundering 1930s, a period Bhana passes over quickly.[20] Charles T. Goodsell, examining the reforms of Governor Rexford G. Tugwell (1941–46),[21] pays more attention to economic and social questions than Bhana and enlivens the personalities of Tugwell and Muñoz Marín, but neither author used the Tugwell papers for the period, and neither adequately accounts for the bitter opposition to Tugwell's policies in both Puerto Rico and the United States. The subfield of Puerto Rican historiography is being cultivated, but it should be further integrated, both within itself and with neighboring Latin American subfields.

Business history is greatly lacking in the field of United States–Latin American relations, but the work of Luis Vallenilla on recent developments in the Venezuelan petroleum industry does not furnish a good example of what is needed. Vallenilla is not a historian but an influential Venezuelan politico-business leader, and he has produced not so much a history of the industry as an exposition of the official viewpoint toward current problems, such as declining production, OPEC (the Organization of Petroleum Exporting Countries), and the nationalization of the Venezuelan industry (which took place after publication of the book). He has no implacable animus against American businessmen of the past, for he knows what made them run and, indeed, has borrowed many of their aims and techniques. They benefited Venezuela in their day, he would say, but that day has passed. His critical faculties are very much present-oriented—against oil companies for grossly underestimating OPEC's power in the early 1970s and against industrial nations for bringing on inflation in Third World countries during the same period. Such attacks, based as they are on Vallenilla's unsupported opinion, cannot be accepted as historical judgments.

Although the book contains neither footnotes nor bibliography, its brief historical account of the petroleum industry seems to be based on readily available government documents. Before the early 1950s it has little but details to add to Edwin Lieuwen's old but scholarly history of the industry.[22] Even the classic "fifty-fifty" tax law of 1948 is not fully set forth. Although Vallenilla devotes considerable space to Venezuela's domestic political history, he largely ignores hemispheric relations and has nothing to say about the crisis of the mid-1960s in Venezuelan-Cuban relations, which followed only a few months after the administration of Rómulo Betancourt established a government oil corporation (CVP) and took an active role in founding OPEC with the Mid-Eastern producing countries. (These last developments Vallenilla naturally discusses at length.) A monograph on the industrial and foreign policies of the Betancourt administration would make a valuable contribu-

[19] Gordon K. Lewis, *Puerto Rico, Freedom and Power in the Caribbean* (New York, 1963; reprint edition, with some chapters deleted, New York, 1968).

[20] Thomas G. Mathews, *Puerto Rican Politics and the New Deal* (Gainesville, 1960).

[21] Charles T. Goodsell, *Administration of a Revolution, Executive Reform in Puerto Rico under Governor Tugwell, 1941–1946* (Cambridge, Mass., 1965).

[22] Edwin Lieuwen, *Petroleum in Venezuela: A History* (Berkeley, 1954).

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tion, and eventually materials will be available for a comprehensive account of Venezuela's evolution toward nationalization and the status of what Vallenilla calls "a truly petroleum nation" (pp. 185 ff.). For this account his book will serve as a kind of primary source.

WHILE THE ORDINARY OBSTACLES to research on United States–Latin American relations are multiplied for the period since 1945, it is well that historians have begun efforts to wrest most of this period from the hands of the journalists. F. Parkinson, senior lecturer in University College, London, at the time of publication, offers an interpretation of Latin America's relationship to the Cold War as part of a semipopular series of social science writings published jointly in California and England.

Briefly, his thesis is as follows: During the first five years of the postwar period, both the United States and the Soviets largely ignored Latin America, but their confrontations in Berlin (1948–49) and Korea (1950–53) forced them to pay more attention. After the first Latin American crisis of the Cold War—that of Guatemala in 1954—most countries of the area embarked on a course of economic development cutting across the *status-quo* policy of the United States. The Cuban Revolution hastened Latin American polarization and led to a crisis period (1960–62) which brought the Cold War to Latin America in earnest, highlighted by the Bay of Pigs, the debate between hard-liners and soft-liners at the Punta del Este conference of foreign ministers, and finally the missile crisis. Cold War influences subsided in Latin America thereafter, despite Cuban efforts to keep them up, especially since the United States' fiasco in the Dominican Republic (1965) and its involvement in Vietnam forced it to maintain a "low profile" (not Parkinson's expression) thereafter.

Despite Parkinson's subtitle, this is not a study in diplomatic history, for he has had access to only scattered public documents, mainly American. It is true that he tries to assume a world view and offers some provocative analytical passages, such as his discussion of the Great Powers' difficulties in penetrating the Third World—"though the absolute military strength of the principles was now unrivalled, their tactical scope was restricted" (p. 38). As another example, he offers an interesting account of how Soviet missiles must have gotten into Cuba. Nevertheless, this is essentially a survey of Latin American attitudes and announced policies, resting largely on published sources: an impressive bibliography of secondary materials, a wide assortment from the Latin American press, and the principal newspapers and periodicals of Europe and the United States—led, for the period 1947–64, by Stanford University's invaluable *Hispanic-American Report*. As a study of reactions and especially press opinions, the work bears some resemblance to Donald M. Dozer's pioneering monograph *Are We Good Neighbors?*[23]

Even granted that Parkinson is studying the Cold War, not hemispheric relations, there is surprisingly slight attention given the United States. The

[23] Donald M. Dozer, *Are We Good Neighbors?* (Gainesville, 1959).

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Alliance for Progress is hardly mentioned, and the climactic events of 1960–62 are subordinated to Latin American reactions. The book illustrates very well the problem of "the one and the many" in writings on Latin America, for Parkinson is always jumping from one country to another, with the result that his writing is choppy and probably difficult for the layman to follow. The most useful features of the book for the general reader are its European orientation—an antidote to the recitations of OAS meetings which have often characterized writings on the recent period—and its temperate attitude toward the United States. The specialist will appreciate Parkinson's many citations of out-of-the-way Latin American newspapers and his long bibliography. We shall see how well his thesis stands up when the archives are opened.

The last two volumes in the group reviewed here represent efforts to synthesize the history of United States–Latin American relations, one of them extending from the late Spanish empire to the present, the other ending with the announcement of the Roosevelt corollary to the Monroe Doctrine in 1904 (presumably to be joined later by a sequel). There is no doubt we badly need such a survey, for during the last thirty-five years only three have appeared that deserve to be called scholarly and comprehensive—one already mentioned by Samuel Flagg Bemis in 1943 and others by J. Lloyd Mecham in 1965 and W. H. Callcott in 1968.[24] None has ever been wholly satisfactory. Bemis solved the problem of tracing the main current of United States–Latin American relations without losing himself in eddies and by-channels, but he often necessarily underplays bilateral relations, and he regularly over-emphasizes the United States side of controversial questions. Also, as already remarked, his book has never been revised in the light of more recent scholarship. (Probably such a task should not now be attempted.) Mecham, also basically pro-American in outlook, is a little more balanced and much more up-to-date in scholarship, but his style is dry, his emphasis mainly on politics and diplomacy, and his organization hopelessly chopped up among topics and countries. Callcott, though full of facts and dates, is essentially a chronicler of events.

The Briton Gordon Connell-Smith, lecturer at the University of Hull, might seem a likely person to attempt the job, with the presumed objectivity of an outsider and considerable past study of the subject. He claims to have thoroughly examined published primary sources for United States policies (and apparently also some unpublished materials), but down to 1945 at any rate, his footnotes refer mainly to standard American monographs. He omits a bibliography for the remarkable reason that he has been unable to examine more than "a proportion" of "the enormous amount of material available" (p. x)—an explanation he would surely reject from an undergraduate.

Connell-Smith's survey of hemispheric relations is wholly unacceptable because of a flagrant anti-United States bias, which more than offsets the

[24] J. Lloyd Mecham, *A Survey of United States-Latin American Relations* (Boston, 1965). Wilfrid Hardy Callcott, *The Western Hemisphere: Its Influence on United States Policies to the End of World War II* (Austin, 1968).

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patriotism of Bemis. Again and again Connell-Smith lengthily presents the worst aspects of American actions (and there are many) without any effort to balance them with such factors as genuine American sympathy and well-wishing (which he thinks hypocritical), insuperable dilemmas faced, domestic opposition, Latin American inadequacies, or European imperialism in the Third World. Connell-Smith occasionally holds up British actions as bad examples, but in general he offers a sanitized version of British influence, both in Latin America and elsewhere. He is a past master of the technique of getting American writers to do his dirty work by quoting the most anti-United States passages of their more balanced studies, sometimes out of context. Indeed, Bemis and Mecham serve him a double purpose; when they defend American actions, he cites them as examples of conservative, self-centered American thought; when they offer criticism, he implies that if even they recognize the shortcomings, no other proof is really necessary.

Such a sweeping condemnation by a reviewer requires substantiating examples. Connell-Smith quotes from Albert K. Weinberg that John Quincy Adams was "so ardent an advocate of Indian dispossession" but omits Weinberg's following statement that Adams "had previously conceded that cultivated Indian lands would always be respected" (p. 53). The discussion of Monroe's delay in recognizing new Latin American nations ignores the complexities of the European situation, the Holy Alliance, and indistinct British policy.

The pages covering the period 1860–1914 are perhaps weakest of all. Although American writers such as Walter LaFeber have offered a many-faceted analysis of Grover Cleveland's Venezuelan policy in 1895,[25] Connell-Smith limits his explanation to Cleveland's personality and political concerns and the propaganda of William L. Scruggs. He offers an oversimplified account of the Panama Revolution of 1903 and the ensuing canal treaty wholly in terms of Theodore Roosevelt and Philippe Bunau-Varilla. For this Connell-Smith's only supporting citations are a Roosevelt letter characterizing the Colombians as "the Bogotá lot of jack rabbits" and Bemis' concession that the episode was "a great big black mark" on the history of the United States' Latin American policy (p. 104). Connell-Smith exaggerates the importance of the American-controlled San Domingo Improvement Company to make the American customs receivership seem largely an effort to secure money for New York bankers, citing as sole source a passage from Roosevelt's instructions quoted in Munro, *Intervention and Dollar Diplomacy*, but omitting Munro's careful explanation of other factors motivating the president. Later, in discussing Wilson's Dominican policy, Connell-Smith devotes a whole paragraph to James M. Sullivan's unfitness to be American minister—undoubtedly true but a trivial factor in the complexities of Wilson's policy. Similarly in Mexico, after a wholly inadequate phrase or two about the problems of American economic penetration under the regime of Porfirio Díaz, Connell-

[25] Walter LaFeber, *The New Empire, An Interpretation of American Expansion, 1860–1898* (Ithaca, 1963), pp. 242–59. In these pages LaFeber summarizes the principal earlier interpretations and sets forth his own.

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Smith quotes all of Elihu Root's fulsome eulogy of the dictator, presumably as a sample of American comment (although he notes that Bemis thought it exaggerated). Did Connell-Smith never hear of John Kenneth Turner?

For the period since 1945 the book is slightly more useful, as Connell-Smith borrows considerably from his earlier study of the inter-American system, a distinctly better-documented work.[26] As a result, however, the history of recent relations is told largely through the story of OAS meetings, punctuated by crises in which the Americans (usually justly) come off badly. Some distortion remains—for example, he gives twice as much space to the squabble over the anti-Communist resolution at Caracas in 1954 as to the missile crisis, although conceding the great danger posed by the latter. Similarly, the Guatemalan crisis of 1954 gets more space than the whole history of the Alliance for Progress.

Even more incomprehensible than Connell-Smith's virulent Yankeephobia is his almost complete inattention to economic factors before the 1930s. One might think that the history of United Fruit (about which Americans have written a good deal) would furnish him with valuable ammunition, but he does no more then mention its incorporation and influence on United States official policy (unsubstantiated by any citation) and its role in the background to the Guatemalan crisis of 1954. He manages to discuss United States-Cuban relations without referring to the sugar trade or American investments on the island until page 170, where a footnote mentions connections of Hull and Adolf A. Berle with sugar interests. Even after 1945 there is little attention to economics; Volta Redonda is passed off curtly in one sentence.

Connell-Smith's underlying thesis seems to be that from the beginning the United States sought unchallenged power in Latin America, as contrasted with European desire for a balance of power—not a wholly unreasonable thesis, but he makes the United States' drive more conscious and consistent than it actually was. He believes that the Monroe Doctrine clearly and intentionally foreshadowed American hegemony (so do some American writers) and regards the Roosevelt corollary as completely consistent with the long-range implications of the original doctrine. Also he seems to equate the post-1898 thrust into the Caribbean with the simultaneous American advance into the Far East without observing the basic differences between the two processes. But, lacking a comprehensive discussion of hemispheric economic forces, his work never makes clear the motivating spring which drove American ambitions. Was it sheer, sublime self-confidence, greed for power, a belief in an American master-race? In his ill-rationalized bias against the United States Connell-Smith seems to have exhumed the devil theory of history.

Where Connell-Smith writes with acid in dealing with United States–Latin American relations, Lester D. Langley's account is more bland. He offers a readable but somewhat uneven survey of American-European rivalry in the Caribbean and Mexico, presenting no pronounced thesis but briefly analyzing American motives and views at the end. (One of these views, however—

[26] Gordon Connell-Smith, *The Inter-American System* (New York, 1966).

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that the Caribbean was a laboratory for Progessive ideals—is little developed in the body of the book. Presumably it will bulk larger in the sequel.) Langley maintains a much better balance than Connell-Smith; but he probably ought to be a little more severe with some of the early American expansionist maneuvers, especially the unscrupulous Florida policy of the Jefferson, Madison, and Monroe administrations. The book contains islands of apparently original research in primary sources, surrounded by material based on standard monographs—an unobjectionable quality in a survey like this. Langley's best sections are very good—for example, those dealing with the Cuban question in the 1820s, the Texas question, the Central American background to the Clayton-Bulwer treaty, the events of the French Intervention in Mexico, the Santo Domingo and canal questions after the American Civil War, the European background of the Spanish-American War, and the Venezuelan crisis of 1902.

If the whole book matched these sections, it could be highly recommended as a basic reading assignment for an undergraduate course in American diplomacy or relations with Latin America and as an introduction for the general public. It is partly disqualified for such use, however, because of jumbled organization at the beginning. Dividing early hemispheric relations into separate chapters on territorial expansion and trade means that the Louisiana Purchase must be discussed before and entirely apart from Toussaint L'Ouverture, who was a vital factor in making the purchase possible. Also, there are surprising omissions and superficial sections. Considering that Langley has already written an excellent brief survey of American-Cuban relations, one would expect him to pay more attention to Cuba; it is not fair simply to refer the reader to the earlier book. He devotes almost as much space to the minor Yucatán question as to the Mexican War. He omits any reference to the Tehuantepec transit route after 1860. He possibly overemphasizes the Venezuelan crisis of 1895 but does not even mention Blaine's clumsy negotiation with Haiti for a naval base at Môle St. Nicolas four years earlier. Most striking of all, he ends the book in 1904 but leaves out the Panama revolution and canal treaty of 1903, after having discussed earlier canal projects and the Hay-Pauncefote negotiations.

Two basic shortcomings are difficult to explain. Like Connell-Smith, Langley has undervalued American economic expansion into the Caribbean. He has little to say about the Cuban sugar trade in any period or about the American acquisition of sugar lands on the island after the Ten Years War. (His earlier book about Cuban-American relations is weak on this score too.) For United States-Mexican relations the enormous flow of American capital into Mexican railroads, mines, and landholding after 1875 was more important than the French Intervention; yet Langley virtually ignores it. Other economic ventures which deserve more attention are tropical agriculture on Santo Domingo and the continental coastline and steamship lines connecting the Caribbean with the United States and Europe. Tariff problems and efforts to negotiate reciprocity treaties are important enough to outline.

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Finally, Langley shows fitful attention to American-European rivalries in the area—a major theme, to judge from the title of the book. The Louisiana Purchase is a first-rate example of competing forces, all of which should be more carefully analyzed. (Much the same may be said of Pinckney's treaty.) Langley sets forth the Anglo-American rivalry in Central America effectively up to the Clayton-Bulwer treaty of 1850; thereafter the coverage is thinner. The brief account of Cuba in the 1850s omits the Ostend Manifesto entirely. There is almost no discussion of British interests in Mexico between the 1820s and the 1860s; and the satisfactory treatment of the McLane-Ocampo treaty should be balanced with at least some reference to the Mon-Almonte treaty, which created a virtual European protectorate over the conservative government in Mexico City during the War of Reform.

A book containing fewer than two hundred pages of text and covering a vast, complex subject like this cannot include every critic's favorite interpretations or illustrations. Nevertheless, one must conclude that the search for a survey to replace Bemis, Mecham, and Callcott is not over.

THE EXAMINATION OF NINE BOOKS such as these, dealing with a wide range of hemispheric relations in a variety of ways, suggests several general statements about priorities in the future development of the inter-American field. Hitherto political relations have been emphasized, as in the case of the books by Munro and Bhana, largely because a major source of primary materials, the published documents and archives of the United States government, is easily accessible. While continuing to fill gaps in the history of bilateral diplomatic relations between the United States and Latin American nations, historians must now emphasize economic, social, and cultural relations as well, especially the first named. In some cases—for example, Connell-Smith and, to a lesser degree, Langley—they must begin by recognizing the importance of economic factors and using to the full monographs already available. But many more economic monographs are wanted, especially studies of individual businesses. The National Planning Association made a promising beginning with its series "United States Business Performance Abroad"[27] (not all devoted to Latin America), but these studies emphasized recent developments and were little documented. We also need monographs on the history of trade and communications and of government-business relations, both in the United States and in Latin America. The Steward and Hilton works of the collection reviewed here show how much can be done even without access to business archives, using government documents, private papers, and other types of materials already available.

In the area of sociocultural relations, the contrast between the treatment of

[27] Richardson Wood and Virginia Keyser, *Sears, Roebuck de México, S. A.* (Washington, 1953); Eugene W. Burgess and Frederick W. Harbison, *Casa Grace in Peru* (Washington, 1954); Wayne C. Taylor and John Lindeman, *The Creole Petroleum Corporation in Venezuela*, with the collaboration of Victor López R. (Washington, 1955); Stacy May and Galo Plaza, *The United Fruit Company in Latin America* (Washington, 1958); Theodore Geiger, *The Electric Company in Brazil* (Washington, 1961).

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the Haitian occupation of the 1920s by Munro and Schmidt suggests that even though relying on United States official materials, the historian need not confine himself to Government House but may report from the dusty streets and outlying farms as well. Most immediately, other historians should apply Schmidt's methods to the remaining protectorates of the early twentieth century. Still other researchers, spreading out into business records and local or regional Latin American sources, might well study oil and mining camps, banana plantations, and the environs of each, as well as the regions served by American-operated railroads for evidence of sociocultural impact. Another area of inter-American relations which needs attention is that of popular reactions to controversial issues. For the Mexican War period the writings of such as Brack, Velasco Márquez, and Schroeder have laid the groundwork for a synthesis. Few other controversies generated so much and such accessible public writing on both sides as the Mexican War; most of these will be more difficult to study.

Finally, whether in monographs or surveys, historians must approach the subject of United States–Latin American relations with more balance and sophistication than sometimes in the past. Bemis' mixture of complacent and anxious patriotism reflected American public attitudes of the Good Neighbor period and World War II, just as Connell-Smith's Yankeephobia doubtless owes something to Vietnam and European resentment of the superpowers. It is time now to treat United States–Latin American relations as a set of complex historical processes, a combination of determinism and limited choice, in which manifest destiny contended with *noblesse oblige,* and thin-skinned nationalism with both. Probably we cannot prevent some historians from reading a moral out of nearly two centuries of hemispheric contacts between the one Strong and the many Weak. It is not too much to ask, however, that these moralists shape their praise or censure to fit the available evidence.

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# Reviews of Books

## GENERAL

ARNOLD TOYNBEE. *Mankind and Mother Earth: A Narrative History of the World.* New York: Oxford University Press. 1976. Pp. xi, 641. $19.50.

This is A. J. Toynbee's last book, finished in 1973 and published posthumously. Its principal interest lies in the ways Toynbee's thinking developed after he wrote his famous *A Study of History*. In this work, for instance, his introductory and terminal chapters focus on man's relationship with the biosphere—a concept absent from his earlier thought. Throughout he exhibits more interest than before in art as a supplement to literary evidences of historical relationships.

As the title suggests, Toynbee announces his grand theme to be the relationship between mankind and the physical environment—personified as Mother Earth. He declares that the central issue of human history is a moral one: whether mankind will transcend greed for material wealth before struggles for possession of such wealth lead to destruction of the biosphere itself. Just how this increase of "spiritual potentiality" (p. 575) might occur is left vague: acceptance of "precepts, preached and practiced by saints that hitherto have been regarded as being Utopian counsels of perfection" (p. 20) is as close as he comes to defining how cataclysmic destruction of Mother Earth can be avoided.

Yet though he declares this to be the central theme of his book, it turns out otherwise. Toynbee's encyclopedic acquaintance with human history is instead compressed into eighty-two separate chapters, some as short as three pages, in which brief, staccato statements replace the discursive style that lent suggestiveness to so many passages of *A Study of History*. A surprising proportion of the whole text consists of dry-as-dust political chronicle, heavily burdened with proper names. Chapters on religious, intellectual, and cultural history occasionally interrupt the main stream of war and politics without being effectively integrated into it, since they, too, follow the chronicle format.

The proportions of the book will surprise most readers. Mid-point almost coincides with the Christian era, and the last two centuries require only twenty-seven pages. Toynbee clearly means to tell us that world history since 1763 bulks no larger in the overall record of mankind and Mother Earth than do the achievements of Sumer and Egypt before 2181 B.C. which occupy almost the same number of pages. After so many details of ancient state-building it is perhaps tonic to find world politics since 1763 condensed into the brief compass of about half a dozen pages (pp. 566–68, 576–80).

Toynbee's *obiter dicta* and idiosyncratic judgments are often arresting and sometimes illuminating or provocatively suggestive. But such scintillations are overwhelmed by the chronicle, and because I do not share all of his likes and dislikes, his epithets often struck me as arbitrary. Thus for example he calls the Assyrians "brutal" and "demonic," whereas the Persians were "enlightened" and "tolerant." (When they punished rebels ruthlessly, as the Assyrians had done before them, it was because the subject peoples in question were "irreconcilable.") He is kind to Mohammed, yet denounces Judaic and Christian intolerance. He praises Peter the Great for being a practicing technician who worked with his own hands (p. 550); yet the industrial revolution results when "regimentation already imposed in military parade grounds was applied to civilian factories" (p. 562). Being inspired by greed (p. 564), it lured humankind along the road to destruction "by a greed that is now armed with the ability to defeat its own intentions" (p. 574).

The classical historians of Greece and Rome habitually attributed political events to "greed," "pride," "honor," or some other moral condition of mankind. Perhaps in this, his final work, Toynbee returned, whether consciously or not, to the classical frame of mind that so powerfully informed his youth. The unexpected political focus



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and the moral judgments that dominate the book certainly suggest an intellectual kinship with the historians of classical antiquity that is shared today by very few.

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PETER GAY. *Art and Act: On Causes in History—Manet, Gropius, Mondrian*. New York: Harper and Row. 1976. Pp. xv, 265. $15.00.

Historians and art historians, as well as the informed public, will welcome Peter Gay's latest volume, *Art and Act*. The book is significant not for its novel interpretations of Manet's, Gropius', or Mondrian's *oeuvre*; the discussion of these men contains no startling stylistic re-evaluations. The importance of the book stems rather from the author's oft-stated plea for a more catholic perception of historical causation—for a perception which takes into account the variety and fecundity of historical possibilities and their intended and unintended consequences.

The work is based on a series of lectures given at Cooper Union in 1974, and thus the author rightly assumed that his audience had a fair familiarity with the three protagonists of his study. Each essay asks the reader to gaze beyond the canvases and edifices and to contemplate the multiple forces at work which result in an artist's esthetic statements. Gay classifies these under the headings of culture, craft, and privacy. By analyzing these forces and how they interact on both conscious and unconscious levels, the historian perhaps can partially comprehend the baffling concepts of creativity and change.

Gay also seeks to dispel some of the major myths which popular accounts of modernism in the arts have promoted. In the piece on Manet, for example, he appropriately remarks that "the principal component in Manet's modernity was his emphatic acceptance of the contemporary world" (p. 104). The commonly-held assumption about avant-garde artists as adversaries of a bourgeois culture is effectively refuted throughout the book. Gay affirms as well the essential historical complexity of an innovator's role in his milieu—a role which often sought to strip away the subjectively perceived obsolete models of his craft while preserving the best of both the past and the present. T. S. Eliot (whom Gay cites in a footnote) held this view in a lovely essay on "Tradition and the Individual" (1919) in which he wrote, "Tradition is a matter of much wider significance. It cannot be inherited and if you want it you must obtain it by great labor. It involves in the first place, the historical sense . . . not only of the pastness of the past, but of its presence."

Eliot's admonition neatly applies to at least two of the modernists in *Art and Act*. Manet, while absorbing the traditions of a Velasquez or a Watteau, recast these esthetic influences into modern painterly language which, in retrospect, seems more appropriate to mid-nineteenth-century European culture than the painstakingly realistic academicism of a Bouguereau or a Couture. Eliot's exhortation also fits the architectural *Weltanschauung* of Gropius. He created significant and suitable environments for an industrial and technological age while simultaneously wishing to abandon the self-conscious historicist kitsch so prevalent in early twentieth-century architecture. Yet he reflected upon and was shaped by the works of both predecessors and like-minded contempories (such as Schinkel and Behrens). The animating philosophy of the *Bauhaus* as a laboratory of collaboration of artists, craftsmen, and architects also attested to the dream of seeking a reworkable past.

Mondrian's stark pictorial abstractions added another facet to the problem of historical causation. For while Mondrian, perhaps even more stridently than Manet, affirmed the value of modernity—he likewise sought to repress and submerge his fear of unruly nature by increasingly simplifying his canvases of all but the most basic lines and color. Gay, like others before him, sees in this process a private and often passionate attempt of sexual self-denial—a spectacular process of sublimation transposed on a two-dimensional plan.

Thus, as we have come to expect from previous works of the author, *Art and Act* is a well-documented, felicitously written, and sensibly organized volume which asks the right questions about art and about history. Yet it leaves the door open for an assortment of complex answers.

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RICHARD RULAND. *America in Modern European Literature: From Image to Metaphor*. New York: New York University Press. 1976. Pp. xv, 197. $12.50.

Richard Ruland knows the limitations of his book. He admits that a short study of a topic as broad as the changing images of America based on a body of evidence as large as European literature can hardly be definitive. Only a very small sample of the literature can be examined, and the analyses of individual authors must be brief, emphasizing the authors' uses of America. Thus it is not surprising that Ruland's selections sometimes seem arbitrary and his capsule discussions sometimes seem superficial and reductive. Nevertheless, many of his examples are very convincing, demonstrating an en-

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cyclopedic knowledge of Russian, German, French, and Italian literature. Furthermore, his brief glimpses at particular writers are often concise and provocative.

But Ruland's triumphs over the occupational hazards of writing a broad literary survey do not per se answer an obvious question: of what use is this study to historians? Ruland emphasizes that he is primarily interested in the esthetic uses of America. Still his book should be quite valuable to students of American and European attitudes. Part one offers a concise overview of the evolving images of America presented in European travel, immigration, and creative literature from around 1500 to the early twentieth century. This survey is based primarily on secondary sources. (One addendum to his bibliography should be Evelyn Page's *American Genesis*.) Ruland outlines several familiar images: America as a virgin land of riches and the home of the Noble Savage; America as a model of political freedom and economic opportunity; and America as the technological future of Europe and the world. Part two is more "literary" than part one. But it should also be useful to historians since both Ruland's survey of European literature from 1900 to 1950 and his examination of six well-known authors (Mayakovsky, Kafka, Brecht, Soldati, Green, and Perse) demonstrate the remarkable endurance of early concepts of America as well as the wide variety of uses modern European authors have found for America.

The latter topic is especially fascinating. The American metaphor can take the shape of Gorky's one-dimensional, bloated capitalists, or ironic dreams about Virginia gentlemen in Berger's *The Garden*. In *Amerika* Kafka can paint the gigantic inhumanities of our urban technocracy while still grasping for the revitalizing powers of the West. Brecht depicts a nightmare of debauchery in his City of Mahagonny, while Soldati and Green often explore particularly American strains of sexual repression. In other words, America has become the European authors' Great White Whale. The outlines and associations are familiar enough to recognize, but the subject is so immense and ambiguous that the creative imagination is free to project an almost infinite variety of hopes and fears about the past and future, Europe and America. Thus, Ruland's survey suggests that whenever modern European authors use America, they are revealing as much or more about themselves as they are about the New World.

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GEORGE M. KREN and LEON H. RAPPOPORT, editors. *Varieties of Psychohistory*. New York: Springer Publishing. 1976. Pp. 370. Cloth $15.95, paper $8.50.

"The present volume . . . has grown out of our collaborative efforts, beginning in 1967, both to conduct original research in psychohistory and to develop appropriate courses for interested students . . . . [It] provides newcomers to psychohistory with a broad introduction to the various branches of this field: theory and methods, psychobiography, childhood history, and group processes." George M. Kren, who is a historian, and Leon H. Rappoport, a psychologist, may be right. My acquaintance with one of their joint research efforts ("Morality and the Nazi Camps," *Western Humanities Review*, 1972) does not raise my confidence that they are, and all of my experience as a practicing psychohistorian and teacher of the psychological interpretation of history tells me that they are wrong.

What do beginners in psychohistory need? Some knowledge of some psychological theories that are likely to prove useful, some knowledge of how much and what kind of historical data to look for, and the start of an emotional conviction that they do in fact have the right, and can gain the ability, to carry on psychohistorical studies. I suppose that it is useful to teachers to have available a collection illustrating types of psychohistory. These teachers, however, will have to point out to students what is going on in these selections—which theory and data did the writers use, and why? Moreover, they will have to toil mightily to counter the depression that neophytes must feel when confronting such supposedly fine work, work that has been printed and reprinted.

Actually the collection scarcely serves as a valid sample of the outstanding work in psychohistory. (I offered a list of choices several years ago, *Historical Methods Newsletter*, 1973.) The articles that supposedly deal with theory and methods, by Bruce Mazlish, Frank Manuel, and the editors, simply rehash all the old pros and cons of doing psychohistory at all, though the editors' contribution does underline the importance of not restricting theory to psychoanalysis alone.

The section on psychobiography offers one masterpiece, Richard Bushman's article on Franklin's character, the best introduction to theory and practice in psychohistory that I know. Also helpful is a note by the Georges about how they wrote their book on Wilson. For some reason only half the chapter on Hitler from Erikson's *Childhood and Society* merited inclusion. The part devoted to the history of childhood opens with Lloyd de Mause's hypotheses on the treatment of children in the West, very exciting propositions for further consideration, though the editors sometimes treat them as already established. There is a good short essay by John Demos, a solid paper by Michael Goodich, and an "extensive abridgment" of Peter Loewenberg on the Nazi youth cohort, a study distin-

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guished for its daring venturesomeness even though its argument is contested widely.

The last selections, on "Group Processes and Historical Trends," certainly do show that multipersonal behavior can be studied psychohistorically. None of the authors is a historian, so I feel a little less embarrassment at the paucity of striking results that often marks these efforts. Herbert Kelman, for instance, is a distinguished social psychologist, but here he is represented by a speech on unrestrained violence and how it can be cloaked and permitted by, among other factors, the absence of individual responsibility commonly felt when serving entities such as "the government." F. H. Allport analyzed this phenomenon far more profoundly and movingly in his study of Sacco and Vanzetti ("Justice Takes Its Course," *Institutional Behavior,* 1933). The book closes with a diatribe by Clarence Karier against Jung (for a different account of part of Jung's life, see my review in *The Journal of Modern History*, 47:360–63), based in part on facts, but also offered with an eye to disseminating a proposition dear to the editors, that ideological commitment and psychological hangups do not erase the necessity of moral choice.

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CLAUDIO NAPOLEONI. *Smith, Ricardo, Marx.* Translated by J. M. A. GEE. New York: Halsted Press. 1975. Pp. 198.

In this thoughtful but puzzling little book, Claudio Napoleoni considers aspects of economic theory of the Physiocrats, Smith, Ricardo, and Marx. Quesnay, Smith, and Ricardo are represented by brief texts at the end. The opening chapters on the Physiocrats and Smith are useful introductions, and make clear how each theory related to the real economic conditions of the time. The treatment of Ricardo also sheds light on his theory of profit and his views of class relations. It ends up somewhat obscuring Ricardo's views on value, however, because the author is more interested in the problems of the labor theory than in the cost-of-production theory Ricardo actually held. This perspective arises from Napoleoni's overall aim, which is not to write a brief essay on the history of economic theory, but to present certain contradictions in classical theory that Marx's critique was designed to resolve. The author assumes rather than demonstrates the success of Marx's solutions. Unfortunately, Napoleoni does not discuss Marxian economics on the scale he treats the other theorists, so that the reader who does not know already will not learn here how Marx moves from his critique of the logic of classical value theory to an account of real economic relationships. Since Napoleoni acknowledges (in a brief introductory chapter) the existence of unsolved difficulties in Marxian theory, but does not say how they relate to the basic formulations of value and capital, the reader has little chance to grasp any overall argument in his book. Finally, the major point the author tries to establish in regard to Marx is highly suspect, namely that abstract labor produces exchange value only under capitalistic conditions. This seems to forget that Marx attributed differing meanings to basic economic categories at various moments in history, and that he regarded abstract labor as present in all commodity production. I confess that Napoleoni's purpose in denying this is obscure to me.

J. E. SEIGEL
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FERNANDO CLAUDIN. *The Communist Movement: From Comintern to Cominform.* Part 1, *The Crisis of the Communist International.* Translated by BRIAN PEARCE. Part 2, *The Zenith of Stalinism.* Translated by FRANCIS MACDONAGH. New York: Monthly Review Press. 1976. Pp. ix, 304, 91; vi, 305–644, 95. $27.00.

In this first of two projected volumes, Fernando Claudin, a former member of the Spanish Communist Party, provides a devastating critique of the theory and practice of the Soviet-dominated international Communist movement from the perspective of an independent Marxist in the tradition of Luxemburg, Gramsci, and Roy. He argues in part 1, "The Crisis of the Communist International," that Communist leaders including Lenin failed to deal in a theoretically creative way with the changing nature of capitalism in the post-World War I setting, while at the same time they subjugated the Comintern to the national interests of the Soviet Union. The result was a crippling policy of opportunism which during the 1920s and 1930s ignored genuine opportunities for revolutionary action in Germany, China, and Spain. The second part, "The Zenith of Stalinism," levels the same accusation for the period 1944–53 when, Claudin claims, the Communists in Greece, France, and Italy in particular were prevented from properly exploiting the revolutionary mood of the masses by Stalin's policy of dividing the world into spheres of influence with the imperialist United States. Stalin also takes the blame for the sharp decline in the numerical strength of Communist parties throughout the capitalist world in the post-war era. The author cites a broad range of primary sources in Russian, French, Spanish, and English but draws upon a much more limited selection of monographs.

Given Claudin's forthright ideological commitment the general conclusions and the interpretations of specific crises are largely predictable. Still this represents the first comprehensive docu-

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mented treatment of the subject from this point of view and is at times highly suggestive. On its own ground—that of an immanent Marxist critique—it suffers from two shortcomings. It does not provide any significant insights of its own into the general nature of twentieth-century capitalism and its crises. Nor does it make important distinctions among revolutionary situations which the author claims existed wherever Stalin's policy revealed its bankruptcy. Geographical conditions, ethnic diversity, and even shifting class relationships are glossed over in deference to the author's revolutionary enthusiasms. Moreover, it is difficult to reconcile Claudin's definition of a genuine Marxist party—ironically still couched in Eurocentric terms—with his acceptance of socialist revolutions in overwhelmingly agrarian societies.

ALFRED J. RIEBER
*University of Pennsylvania*

DAVID WILKINSON. *Revolutionary Civil War: The Elements of Victory and Defeat*. Palo Alto, Cal.: Page-Ficklin Publications. 1975. Pp. vi, 229. $6.95.

This study explores the determinants of victory and defeat in revolutionary civil wars on the basis of a comparison of eight cases: Rome 83–82 B.C.; England, 1642–46; Mexico, 1914–15; Russia, 1918–20; China, 1926–28; China, 1930–34; Spain, 1936–39; and China, 1946–49. After brief summaries of the eight cases, two chapters are devoted to the political and military factors in victory and defeat, and the last to an analysis of these factors.

David Wilkinson summarizes his findings in terms of ideal types of losers and winners, and comments on the dilemma of moderates and on questions of strategy. The losers—as typified especially by the Chinese warlords, the Spanish Republicans, the Russian Whites, and the Chinese Nationalists—are depicted as having a limited political base, seeking to hold together disparate interest groups, weakened by corruption and incompetence, and lacking in able leadership and military mobility. The victors, by contrast, are more united and dedicated, less corrupt, and committed to goals that command a wide following. As exemplified by the Bolsheviks and the Chinese Communists, this revolutionary zeal leads them toward a politicized totalitarian society. This penchant for autocratic methods also characterizes other victors—such as the Spanish and Chinese Nationalists—who otherwise differ sharply from the Communists. The moderates, for their part, inevitably find themselves in a hostile environment where the compromises they favor are not possible. The strategic challenge of revolutionary civil wars is thus seen essentially as that of competing efforts to mobilize human and physical resources.

At this level of generality Wilkinson's analysis has much to recommend it, but any effort to describe and compare eight revolutionary civil wars in very brief compass has its limitations. What one misses in particular is the extent to which differences in political culture and level of development affect the outcome. Even the six twentieth-century cases occurred in countries (Mexico, Spain, Russia, China) that differ in many essential ways. To stress the similarities of their revolutionary experiences without taking equivalent account of the contextual differences is to miss significant elements of victory and defeat.

CYRIL E. BLACK
*Princeton University*

THOMAS T. HAMMOND, editor. *The Anatomy of Communist Takeovers*. Foreword by CYRIL E. BLACK. New Haven: Yale University Press. 1975. Pp. xvii, 664. Cloth $25.00, paper $5.95.

This work is not, as its title would suggest, a comparative analysis of Communist takeovers. The chapters are not grouped into sections according to whether they deal, for example, with regimes imposed by an army of occupation, created by a guerrilla movement, or brought into existence by pseudo-parliamentary maneuvers, but are rather appended to each other on a chronological basis. Nor is there an introductory chapter attempting to provide an overall theory of Communist seizures of power, which could have served as a common target for the thirty authors involved. Instead, chief editor Thomas T. Hammond provides as introduction a short history of Communist takeovers. Only at the end of the book, in a five-page summary does he undertake a provisional taxonomy of takeovers, attempting to generalize as to the reasons for success or failure.

The definition of "takeover" as used in *The Anatomy* raises other problems. Are we to believe that there has been a Communist takeover in the Indian state of Kerala when its Communist party, twice elected to office in accordance with existing parliamentary procedure, has been twice removed in accordance with the same? And what are we to make of chapters devoted to the flash rising in Hungary in 1956 and the Czechoslovak thaw of 1968? The classification of these events as Communist counterrevolutions appears strained; their significance is rather that the political and economic system imposed by Muscovite fiat suddenly collapsed because even the ruling elites in the two satellites could no longer stomach it. In short, a certain fuzziness of focus pervades the volume.

We should not, however, be too demanding of the authors and editors of *The Anatomy of Communist Takeovers*. The experience of the ACLS Planning

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Group for the Comparative Study of Communism suggests that even where there are funds available for one or more gatherings of the collaborators—which was not true in the present instance—the resultant collective works are often little more than juxtapositions of idiosyncratic essays. Collective volumes do not provide an easy threshold to comparative analysis.

The present work, moreover, covers all successful and most unsuccessful seizures of power which took place prior to 1971. It thus provides a kind of compendium or encyclopedia which will do comparativists (and others) good service as a work of reference. The proclivity of individual authors to survey the most recent literature in the given field increases its usefulness. The most notable instance of this, in my opinion, is an essay by Pavel Tigrid which explains what the revelations of the Czechoslovak thaw of 1968 do for our understanding of the Czechoslovak coup of 1948; this piece may well acquire classic stature. Exceptional contributions are also made by Jürgen Domes on the Communist conquest of China, Dennis J. Duncanson on Vietnam, Boris Goldenberg on Cuba, Nissan Oren on Bulgaria, and Paul Shoup on Yugoslavia, but there are many other high-quality chapters as well.

R. V. BURKS
*Wayne State University*

HERBERT S. DINERSTEIN. *The Making of a Missile Crisis: October 1962*. Baltimore: Johns Hopkins University Press. 1976. Pp. xii, 302. $14.95.

Herbert Dinerstein's study presents an extensive historical background to Castro's revolution, beginning with the Guatemalan crisis of 1954, and treats the Cuban missile crisis as a product of the triangular relationship between the United States, the Soviet Union, and Cuba. The evolution of Castro from a leader of a broadly based (and partially U.S.-supported) movement against the Batista regime to an adherent of "Marxism-Leninism," which he embraced almost two years after his victory, is thoroughly analyzed. The author attributes this evolution to radicalization of the revolution and to growing pressures and threats on the part of the United States, particularly after the breakdown of Khrushchev's "peaceful coexistence" policy following the U-2 episode and the abortive Paris summit conference in May 1960. Moscow's apparently unsolicited commitment to defend the Cuban revolution, with missiles if need be, resulted in the stiffening of Washington's position—and the Bay of Pigs adventure.

Although the author considers the missile crisis within a broad context of the global U.S.-Soviet struggle, he ignores the impact of the bitter Sino-Soviet conflict upon the militancy of the Soviet anti-U.S. posture. Nor does he adequately analyze the compulsion of the Kennedy administration to uphold United States supremacy in confronting globally Soviet-inspired (if not Soviet-directed) forces of "anti-imperialism," from the Congo to Vietnam.

The study is based on extensive research into Cuban, Soviet, and American sources, perhaps too much on sources of an often misleading, propagandistic nature. Its unique value is in its breadth and its historical perspective rather than in the analysis of the crisis itself and of the motivations of the principal protagonists. The book is most valuable to students of Soviet-American relations and of the early stages of the Cuban revolution.

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WALTER LAQUEUR. *A History of Zionism*. Reprint. New York: Schocken Books. 1976. Pp. xvi, 639. $6.95.

Walter Laqueur's *History of Zionism* offers the general reader a useful introduction to the subject. Laqueur has mastered the considerable intricacies of Zionist history to present a readable account of the thought and action of the movement whose concern was "the fate of a sorely tried people and their attempt to normalize their status, to escape persecution, and to regain dignity in their own eyes and in the eyes of the world." The account is enhanced by Laqueur's care in providing the background of modern Jewish and modern European history against which Zionism emerged.

Beginning with an able summary of Western Jewry's emergence from a ghetto existence, Laqueur traces the transition from the optimism of the late eighteenth century, which saw both the emancipation of the Jews in revolutionary France and the growth of more tolerant attitudes elsewhere, to Zionism's despair about the future of assimilation, generated in part by the appearance of racial anti-Semitism less than a century afterward. Although modern political Zionism proper originated with Theodor Herzl's manifesto *The Jewish State* (1896), Laqueur rightly takes account of the long pre-history of Zionism, which was rooted deeply in the Jewish tradition, and was marked during the nineteenth century by a number of little-noticed Zionist treatises that largely anticipated Herzl's own presentation. Until 1917, however, Zionism remained "a somewhat eccentric movement of young idealists," and it was only with Britain's open expression of sympathy for the Zionist endeavor in the Balfour Declaration of 1917, and the subsequent establishment of a mandate over Palestine, that systematic and extensive

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Zionist efforts there could begin. Nevertheless, the years after the First World War saw the growth of determined Arab resistance to Zionist development and a corresponding erosion of earlier British support. Laqueur's account ends in 1948, with the establishment of the state of Israel.

Although by this periodization Laqueur marks a separation between the history of the Zionist movement and the history of Israel, one of the many virtues of this book is the attention it pays to Zionist developments within Palestine, and the reciprocal influence of the *yishuv* (the Jewish settlement) and the world movement. A second strength is Laqueur's ability to sort out the sometimes bewildering complexity of the movement. Just as Zionism was only one of several varieties of Jewish nationalism, so also was there a rich diversity of opinion within the movement, and a considerable degree of factionalism.

At the same time, Zionism had its critics within the Jewish community who argued variously that it was inappropriate, inadequate, and even a liability for the Jews in the Diaspora. All this Laqueur presents with admirable clarity. Turning to Arab opposition to Zionism, Laqueur explores the dangers that Arab nationalists saw in Zionism and gives a fair account of the subjective perceptions of the conflicting parties.

Although the preface describes the book as "the first comprehensive history in English on a comparable scale," it should be noted that Ben Halpern's *The Idea of the Jewish State* (1961), which is somewhat more analytic and less narrative than Laqueur's work, still remains an important point of departure for scholarly inquiry on Zionism.

STEPHEN M. POPPEL
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RUSSELL MCCORMMACH, editor. *Historical Studies in the Physical Sciences*. Volume 4. Princeton: Princeton University Press. 1975. Pp. vii 235. $12.50.

The six articles in this volume constitute a miscellaneous set. Five are sharply focused, highly detailed studies of disparate complex issues in different national contexts of the nineteenth and twentieth centuries. These are similar in style and approach, and merge appropriately with the existing literature (sometimes generated largely by the same author). They contain no surprising conclusions. Any one of the five could be chosen as illustrative of the group. The sixth and last article weaves historical examples into a philosophical discussion arguing for a positive scientific and technological future.

Three of the articles (by Robert Fox, Robert H. Silliman, and Mary Jo Nye, respectively), without deliberate design of the authors, center on similar themes in the history of physics in the French scientific community of the last century: the design of and disagreement about research programs, and the success and failure of these programs. Two of these concern the Napoleonic years and the great age of Laplace, while the third considers an end of the century controversy over the discovery and interpretation of several new types of radiation.

Two other articles deal with Germany and the United States respectively. German scientists in the middle of the nineteenth century were still involved with *Naturphilosophie*, and nowhere was this involvement more apparent than in the new, borderline discipline of biophysics, the subject of Charles A. Culotta's article. In the United States of the World War I period one of the most active theoretical scientific fields was physical chemistry. Studies of the subject—such as this excellent article by Robert E. Kohler—tend to center on one or two persons and one or two developments.

The sixth article requires singling out, because it is likely to be overlooked in this volume. This essay is an interesting philosophical analysis by Boris Kuznetsov of the philosophy of optimism in modern science, through an examination of the relation of understanding and reasoning in classical (i.e., prerelativity and prequantum theory) and nonclassical (i.e., present) science. In Kuznetsov's view these are inseparable in nonclassical science. Kuznetsov employs many examples to illustrate the dynamic quality of nonclassical science (defined as the necessary requirements for re-examining fundamental principles continuously as a normal part of science) and its effect on contemporary civilization, both in terms of the growth of knowledge and the development of our epistemological outlook. The argument, an extension of former discussions by other authors on the revolutionary character of recent scientific developments, offered at a time of intense interest in technology assessment, the moral content of science, and the effect of science in both micro- and macroeconomic systems, includes discussions of the meaning of this development of science on the economics of science and on morality, and concludes with some predictions about technical advancement in the next few decades.

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## ANCIENT

GUIDO MAJNO. *The Healing Hand: Man and Wound in the Ancient World*. Cambridge, Mass.: Harvard University Press. 1975. Pp. xxiii, 571. $25.00.

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Too often, modern works on ancient medicine concentrate on philology and philosophy at the expense of medical practice. Careful analysis (for Greek and Roman medicine) of Hippocratic or Galenic Greek is, indeed, of cardinal importance, and the ingrained philosophies of all classical medicine must be intermeshed with medical practice, but few books deal directly with doctors and their patients. Guido Majno's *Healing Hand* redresses the usual imbalance, and the essential strength of the book is medical. In a lively and readable style, Majno examines an enormous range of cultures (Egypt, Mesopotamia, Greece, India, Rome, and China) from the vantage point of a physician who commands modern technique and theory.

Majno never poses leading questions to the ancient sources in order that the answers will show how much more "advanced" is the twentieth century. He always provides information either directly from the sources, or from experts in the various aspects of his subject, who receive open and frequent citation. The result is a balanced, delightful account of the ancients as they really were. Majno brings modern explanations to various problems of ancient medicine, but always with the clear distinction that these are what *we* would call them. Thus there are fresh insights, from both modern and ancient thinking, on wound surgery, tissue decay, anatomy, pharmaco-chemistry, and the resultant diagnoses and prognoses, illustrated with modern biochemistry, pathology, and physiology. Majno is more at home in his many comparative sections (comparing what Egyptians, Greeks, etc., thought with what modern medicine thinks) than he is with history, philology, and philosophy. The bits of history are jejune, the philology rests upon Littré and others, and the philosophy (especially that of the Pre-Socratics and Aristotle) is often misunderstood. Such weaknesses, however, are minor, and Majno's approach to the ten Hippocratic "cases" is one of the best accounts of Greek medicine ever written; Roman medicine is carefully blended from expert opinions and the sources; and surgical technique receives a brilliant evocation. The book is a storehouse of well-described technical matter; readers will enjoy the sections on myrrh and frankincense, the antibiotic properties of wine, a simplified theory of wound-closure, and the substances called *pharmaka* in classical antiquity. Some of the many plates and drawings are destined to become standard in future volumes on ancient medicine.

Some minor criticisms: Majno's overview of Hellenistic surgery is badly dated, and Erasistratus and Herophilus do not emerge from his pages with the clarity given to his Hippocrates. P. M. Fraser's *Ptolemaic Alexandria* now contains many of the important texts, collected in footnotes. Majno's toxicology suffers from his omission of Nicander and Philumenus. His Galen is jagged and uncertain, and the account of humoral theory in the Roman Empire is off-center, as indicated by Galen's *Black Bile* and the Pseudo-Galenic *Humors*. Finally, Majno falls into the trap laid many years ago by Charles Singer: Majno writes, "After Galen, the history of the wound grinds to a halt for at least one thousand years. Europe sank into the Dark Ages." Oribasius? Leo? Rhazes? Paul of Aegina? Western medieval medicine and pharmacology? The "Dark Age" myth dies hard.

These are minor quibbles about a magnificent book, one of the few "coffee table" volumes that is a physician's commercial, an amateur's delight, and a fundamental research tool.

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ALLEN H. JONES. *Bronze Age Civilization: The Philistines and the Danites*. Foreword by CYRUS H. GORDON. Washington: Public Affairs Press. 1975. Pp. vii, 182. $6.00.

A Semiticized Aegean, with Minoan, Danaan, and Kadmeian descendants of Semitic settlers from the Levant, is the immediate background of Allen Jones' investigation of Philistine and Danite origins. The Philistines are interpreted as the Indo-European "Tribes (φυλαί) of Hestia (Ionic Ἰστίη)" and located in Euboea, while the Hebrew tribe of Dan is identified as a Semitic group that has come full circle: from Egypt to the Argolid as Danaans, and then, as Danunians, back to the East with their comrades the Philistines as part of the Sea Peoples. After the defeat inflicted by Ramesses III, the Danunians followed their Philistine allies into Canaan eventually to become the Danites. The author suggests that several other Hebrew tribes were remnants of the Sea Peoples as well. He concludes his study with an examination of the impact of the Indo-European Philistines upon Hebrew culture, just as earlier he surveyed Semitic elements in Greek civilization.

Theories of Semites in the Aegean are not new, yet it is surprising that Jones directs much of his study to these very conclusions. His assumptions are often rash (for instance, any kind of cow worship is Egyptian in origin), and more than once he sees significance in accounts that are surely Hellenistic in origin. His lack of expertise in matters Aegean does not inspire confidence: for example, he states that the wall paintings and mosaics of Delos betray the island's Bronze Age greatness, when in fact they are Hellenistic. Often it is difficult to understand the author's line of thought. His desire to have Philistines in Canaan as early as the

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time of the Patriarchs is an idea that seemingly counters his theory of their origins.

While it is likely that the Philistines had some Aegean connections, it is most unlikely that they were the "Tribes of Hestia." His derivation suggests that one can apply proper etymological procedures to Hellenistic Greek transliterations of unvocalized Hebrew words. The Φυλιστιείμ of the Septuagint was a concoction of Greek-speaking Jews to render the Hebrew *PLST*; the aspirated form may derive from the most common term used for Philistines in the Septuagint, ἀλλόφυλοι. Further caution is suggested by the fact that Παλαιστίνη, seen as early as Herodotus, represents an alternate transliteration of the same four consonants. The equation of Danaans with Danites is presented with no convincing evidence. It is perhaps indicative of the author's breadth of scope that he does not even mention the Iron Age *DNNYM* of Karatepe in Cilicia, much less consider a connection between them and the Danaans-Danunians.

G. KENNETH SAMS
*University of North Carolina, Chapel Hill*

KEVIN CLINTON. *The Sacred Officials of the Eleusinian Mysteries*. (Transactions of the American Philosophical Society, New Series, volume 64, part 3.) Philadelphia: the Society. 1974. Pp. 143. $12.00.

This study is intended, according to Kevin Clinton, to be "introductory to a *corpus* of Eleusinian inscriptions." When he started editing the texts in his collection, "it soon became clear that many problems connected with the priesthoods could be treated more conveniently in a separate study than in the commentaries on individual inscriptions."

Clinton's scheme is to treat the sacred offices of the Eleusinian cult one at a time, and under each office to present the evidence chronologically, especially concerning the known incumbents. He has sharpened the evidence by providing improved readings and restorations of many inscriptions, which he has obviously studied meticulously. His greatest contribution, however, is in the area of prosopography. The family relationships of the persons who held the sacred offices are thoroughly discussed. Wherever possible, Clinton has labored hard to draw general conclusions. For example, he shows that, during the periods for which we have the evidence of prosopography, the *daduch* was apparently chosen by election and not, as has been inferred from a statement attributed to Aristotle, by lot. His efforts deserve our praise, even if our general knowledge remains sketchy.

There are blemishes. They include rather frequent typographical errors; errors of cross-reference (page 72, note 32, should read "Archias, no. 3."); omission of references, other than indirect (page 14, note 24); and careless reading of evidence (on page 82 the phrase "for life" applies to priests one and two as well as to priest three). There are numerous examples of unclear or misleading exposition. It is not true that the "unnamed hierophant" of page 20 "appears in an inscription . . . at the head of a group of men." Clinton assumes that he headed them, but in what sense? The "dedication to Medeius" mentioned on page 73 is actually a statue of Medeius dedicated to Demeter and Kore. Unfortunately, the evidence is more often summarized than presented. This study would have been more useful as a volume of appendices to the *corpus* of inscriptions originally planned. But even in its present form the work is excellent, a treasure of detailed information and valuable judgments.

ROLF O. HUBBE
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ALAIN HUS. *Les siècles d'or de l'histoire étrusque (675–475 avant J.-C.)*. (Collection Latomus, volume 146.) Brussels: Latomus. 1976. Pp. 288. 1100 F.

The title of this book arouses expectations that the work itself leaves unfulfilled: Alain Hus himself somewhat disarmingly admits that "Il n'existe pas encore d'histoire étrusque" (p. 274). Instead, it is a general survey of Etruscan civilization, concentrating on the archaic period, but with earlier and later periods taken into consideration. Indeed, the chronological limitation seems at times both arbitrary and unnecessary: the account of Volterra frustratingly stops just before the major period of Volterran expansion, and Perugia suffers a similar fate, although elsewhere later evidence, such as the François Tomb, is considered.

The work is divided into four sections concerned with topography, art, religion and daily life, and Etruscan contacts in Italy and the Mediterranean. The first of these, although it contains a useful synthesis of recent opinion, is really too short to do justice to the number and complexity of issues raised, and the accompanying maps are extremely sketchy. Only one has any scale indicated, and plans as vague as those of Vetulonia and Populonia seem to serve little purpose. On the other hand recent excavations are included. Both Gravisca and Murlo are discussed, with Murlo once again appearing as a site of the highest importance.

The section on art, while it finds the author on home ground, is too brief to permit more than a summary discussion of the major monuments. In the section on painting, for example, Hus deals

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with only the most familiar Tarquinian tombs, and each tomb is allotted a single paragraph. Here, incidentally, the plates, elsewhere serviceable, are less helpful since the photographs of the tombs seem old and faded, and in one case, curiously misleading. Although the author's discussion of the Tomb of the Bulls on page 133 refers to the erotic episodes above the central panel, plate XXXVIII shows the tomb with the two groups blotted out—an odd example of censorship and one that makes the text meaningless.

The third section summarizes, without replacing, Heurgon's book on the same topic. The final chapter discusses a wide variety of aspects of the Etruscans' relations with their contemporaries, from Pythagoras' possible influence on Etruscan religion (not very convincingly argued) to Etruscan finds in Provence and Languedoc.

On the whole the book is a useful, if hasty, summary of the present state of Etruscology, with an impressive display of knowledge of recent bibliography in the footnotes, but without the completeness or the authority of, for example, Pallottino's "Etruscologia." Its use as a text will probably be limited by its price.

JOHN J. REICH
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JOHN ALEXANDER. *Yugoslavia before the Roman Conquest.* (Ancient Peoples and Places.) New York: Praeger Publishers. 1972. Pp. 175. $12.50.

MIRON CONSTANTINESCU *et al.*, editors. *Relations between the Autochthonous Population and the Migratory Populations on the Territory of Romania.* (Bibliotheca Historica Romaniae, Monographs, number 16.) Bucharest: Academici Republicii Socialiste România. 1975. Pp. 323. 30 L.

The first of these two works chronologically surveys the prehistory and early history of Yugoslavia down to the Roman conquest in the reign of Augustus. Based upon archeological evidence—much of it recent and reported in scattered publications and in various languages—this work fills the need for a concise, accurate, and up-to-date synthesis of the information available for the study of the pre-Roman period of Yugoslavia's history.

John Alexander includes useful analogies to archeological discoveries in the neighboring countries of Bulgaria, Hungary, and Romania in the descriptions of characteristic sites. Profusely illustrated, this work complements Wilkes' *Dalmatia*, a recent scholarly study of the Roman period.

The second work consists of twenty-one essays, each by a different specialist, dealing with migratory populations that substantially affected historical developments in the area of present-day Romania from the sixth century B.C. until the fourteenth century. Considered collectively, these essays attempt to demonstrate the survival and continuity of the indigenous population over the span of two thousand years in the face of many seemingly overwhelming forces. This is a challenging thesis that must in some details rely upon supposition and conjecture for lack of concrete evidence. For example, proof of the continuity of the Daco-Roman population after the withdrawal of Roman troops in A.D. 271 remains controversial and open to various interpretations, despite Daicoviciu's argumentation (pp. 51–2). It is a reasonable presumption that a Daco-Roman population assimilated with Slavs in the sixth to ninth centuries, but supporting evidence is meager. Rusu, in his discussion (pp. 123–53) of Avars, Slavs, and the Romanic population in the sixth to eighth centuries readily admits that the archeological evidence is still lacunary in spite of the significant contributions to our knowledge of this period made by investigations over the past twenty-five years. Controversy still surrounds the ethnical ascription of finds dating from these centuries.

To many historians the use of Marxist socioeconomic theory to explain historical phenomena in ancient societies does not appear very sensible. References to "exploitation" of the working class by the ruling class or to "modes of production" will not, however, seriously detract from the value of this work. One detrimental flaw, though, is the tedious English style resulting probably from a too literal translation of the Romanian text. Run-on sentences and sentence fragments abound, as well as instances of incorrect grammar and of nonsensical or peculiar phraseology. Curiously, the translators are not identified. The inclusion of a chronological table would have facilitated the use of this book, but copious documentation of sources should prove of value to specialists interested in the ancient and Byzantine periods of Balkan history.

DONALD W. WADE
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BEZALEL BAR-KOCHVA. *The Seleucid Army: Organization and Tactics in the Great Campaigns.* (Cambridge Classical Studies.) New York: Cambridge University Press. 1976. Pp. xi, 306. $14.95.

"The composition and organization of the Seleucid army are very little known to us." So wrote Rostovtzeff fifty years ago in the *Cambridge Ancient History*. Our documentation is scanty, particularly when measured against Ptolemaic profusion. Seleucid tactics too are obscure and often seem a pale travesty of Alexander's. Such are the problems addressed by Bezalel Bar-Kochva. He sifts

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the evidence carefully, applies common sense relentlessly, and interjects rhetoric judiciously.

First he tackles the organization of the army. In general, he says, the large numbers reported in our sources are reliable, and if analyzed can give us the manpower potential of the military settlements: 47,000 heavy and medium infantry (nearly all Graeco-Macedonians) and 8,500 cavalry (largely Iranians). To them must be added mercenaries, allies, and subjects. The Seleucids avoided the Ptolemies' blunder, and never armed the natives of the Syrian heartland. The key to the whole system lay in the Guards—the Silver Shields (10,000 Greek foot), the *Agema* (1,000 Median horse), and the Companion Cavalry (1,000 Greeks)—recruited from the offspring of the military settlers. They remained on duty until they inherited the family allotment; then, trained and indoctrinated, they went on reserve. All this, so easy to summarize, is established only after strenuous debate.

Bar-Kochva then takes us through eleven battles: two of Seleucus Nicator, seven of Antiochus the Great, and two against the Hasmonaeans. Some are old favorites. On others Bar-Kochva's autopsy sheds a flood of new light. He identifies the sites of "Cyrrhestica," the Porphyrion pass, Panion, Beith-Zacharia, and Elasa. We all know that the Seleucid monarch regularly led his own army, and that he used elephants as a cavalry screen. Bar-Kochva notes other new practices: the adoption of the Parthian cataphract charge, which broke the Egyptian left at Panion and a Roman legion at Magnesia; the use of a reserve for offensive purposes (Ipsus, Apollonia); the sham cavalry retreat to separate the enemy horse from his phalanx (Ipsus, Elasa); and the sending of a small force into a defile as bait for the ambush above (Mount Elburz, Beith-Zacharia).

Even the commander's intent is accessible, and explains the hitherto inexplicable. Two Seleucid disasters took place because their victorious cavalry pursued too long. Why? Not just hot blood, says our author, but a calculated plan to demoralize the enemy—at Raphia by killing Ptolemy, at Magnesia by capturing the baggage. Both strategems misfired.

This is a brilliant book, exciting and ingenious. Its cleverness may be the chief flaw. Ancient actors were not invariably rational, and sheer logic cannot take the place of missing evidence.

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ÉTIENNE TIFFOU. *Essai sur la pensée morale de Salluste à la lumière de ses prologues.* Montreal: Les Presses de l'Université de Montréal. 1975. Pp. 612. $32.00.

This very large book, a typical *thèse,* combines the merits and the defects of that class of work. It is crammed with learning, presenting a full overview of scholarship on the subject, but, for ease of reading and consultation, cries out for the ruthless hand of an editor; much could have been pruned, and the whole reduced in size by at least a third. The author's conclusions, well-argued and supported, will occasion little surprise; he finds that Sallust's prologues are an integral part of the works that they introduce and forcefully express the ideas on which the monographs proper largely depend. Similarly, the prologue of the *Historiae* is crucial for proper understanding of that work.

Among Tiffou's main points are the following. Sallust emphasizes the importance of *virtus* and *animus*; the former is identified with *gloria*. The principal source of his thought is Roman, without significant impact from Greek literature. He presents a new concept of *otium* at Rome, which is raised to the level of authentic *negotium*. The prologues of the *Catiline* and the *Jugurthine War* are different from those of Thucydides, Livy, and Tacitus, because they give the subject and its justification a secondary place in order to furnish a transition between the prologue and the work proper. But the prologue of the *Historiae* is more like theirs; further, it is deeply pessimistic, though this initial pessimism moderates at the end. The *Historiae* is a total rejection of Roman optimism and feeling of destiny. The *Catiline* is a moral, not a political work, which interprets events on the basis of an ideal expressed in the prologue. But *fortuna* can overcome *virtus,* which in the prologue is called invincible. Cato is Sallust's true hero; he incarnates ancient *virtus;* all others have some fault, such as *superbia*.

Much of Tiffou's energy and space is consumed in debate with other scholars, chiefly, among contemporaries, Leeman, Büchner, Syme, Earl, and La Penna. That could have been much abbreviated, for frequently Tiffou concludes such a survey by supporting the view of one or another. I doubt that many English readers will wish to read this book to discover what others have said; Tiffou's subject, in its narrowest sense, is also D. C. Earl's in his *The Political Thought of Sallust* (1961), which I think gives far better value. Tiffou has done good service by providing a conspectus on an important subject, but he has not produced a book that will be considered epoch-making.

HERBERT W. BENARIO
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COSMO RODEWALD. *Money in the Age of Tiberius.* Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 154. $16.00.

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In A.D. 33, a severe economic crisis gripped Rome. For reasons largely unknown, imperial policy changes and/or conditions of the marketplace had precipitated a tumultuous situation distinguished by rampant usury, currency shortages, wild land speculation leading to a collapse in prices, and extensive litigation involving debt defaults. Not surprisingly, given a tradition of avoiding such mundane concerns by Roman *literati*, the events of this year are incompletely treated in extant sources. Tacitus, in the *Annals* 6.16–17, discusses them briefly; Cassius Dio, 58.21.1–5 and Suetonius, *Tiberius* 48.1 offer only passing references. Nonetheless, conditions in Italy and perhaps elsewhere were extremely tense and potentially explosive before Tiberius moved to restore stability through a massive imperial loan program.

This crisis provides an excellent departure point for a study of the monetary policies of the early years of the Julio-Claudian emperors. Such is the intent of Cosmo Rodewald in his short work (71 pages of text), *Money in the Age of Tiberius*. Through a detailed exegesis of the above-mentioned passages, he attempts to explain the reasons for the monetary crisis, to explore its ramifications, and in so doing to present some observations on various fundamental questions of Roman monetary policy and economics: volume of currency issues, imbalances of trade, degree of centralization of monetary control, etc. At the same time, his book is a rebuttal of an earlier article by Tenney Frank dealing with the same topic, "The Financial Crisis of 33 A.D." in the *American Journal of Philology* (56 [1935]: 336–41), which has enjoyed fairly wide acceptance.

Predictably, Rodewald's treatment is far more complete than the short article he refutes. The author seems quite at home with the massive scholarly tradition relating to the many questions which his investigations broach. He seems particularly strong when treating the coins and coinage of the northern military frontiers, and less so when dealing with material from the eastern Mediterranean. But then the crisis on which he focuses appears to have been essentially an Italian or possibly a western one. As complete as his research and critical apparatus are (sixty-five pages of footnotes, five tables, three appendixes), he cannot go beyond his evidence. Herein lies the frustration in working with any aspect of monetary policy in the Roman world. Literary sources are practically nonexistent. For the most part, data are provided only by the coins themselves. With such a tenuous base, definitive conclusions are impossible; speculation is more the order of things. In essence, this study can be little more than a new set of conjectures offered as replacement for those presented earlier by Frank. Rodewald's suppositions pay close attention to pertinent sources, but many of his interpretations are novel and open to legitimate challenge. Regrettably, owing to space limitations, I cannot support this analysis in detail here.

Historians who proceed with caution will be served by this work, but not to the extent promised by its title. Numismatists will find much that is provocative, particularly the author's comments on the *aes* coinage. Prospective purchasers of this book, however, will find it too highly priced.

ROBERT L. HOHLFELDER
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MICHAEL GRANT. *Saint Paul*. New York: Charles Scribner's Sons. 1976. Pp. 250. $14.95.

Michael Grant is an incredibly facile writer whose many books on the ancient Greek and Roman worlds are known to millions of readers. In his latest book, using much the same method that he employed in his *Herod the Great* (1971) and *The Jews in the Roman World* (1973), Grant brings together data from Greece, Rome, and Israel on Saint Paul, who was "a Jew who wrote in Greek and was a Roman citizen" (p. 2). The aim of the study is to show that "Paul is one of the most perpetually significant men who have ever lived" (p. 1). As a historian Grant is concerned to bring out the many-sided aspects of an individual who, in his analysis, was chiefly responsible for making an ecumenical religion out of a nascent Jewish Christianity and who exercised enormous influence beyond the field of religion, in such areas as politics, sociology, war, philosophy, and intellectual and cultural life.

Because Paul in his *Acts* and *Letters* wrote the earliest extant Christian documents, we can study the origins of the early Christian Church and get some tantalizing glimpses of Paul the man. Though Grant is quick to point out that he himself is not a theologian, and that his main focus is on the man of peculiar gifts who profoundly influenced people of widely differing beliefs, races, and times, nevertheless, unavoidably, there is a good deal of time spent describing and expounding Paul's theology and its impact. Grant also deals with Paul's four missionary journeys and his conversion on the road to Damascus. Paul, through his own incredible energy and determination and in his radical thought, by his insistence on the Crucifixion *and* the Resurrection; by his challenge to traditional Judaism and early Jewish Christianity; by his awareness of the incredibility of what he taught; and by his stress on the necessity of faith confronted his contemporaries with some startling thinking and teaching.

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Despite Grant's claim that it is the person of Saint Paul that he wants to present and clarify in his six full chapters, which are supported by notes, a chronological table, maps, a glossary of ancient writings and terms, and an index, there is no way of divorcing St. Paul from his teaching and thinking. What most impresses Grant as historian and the reader is that Paul was a forceful advocate of radical change, of total change, and that he was quite prepared for it and receptive to it. Though his vision in certain areas was limited by his own historical times (for example, slavery, the position of women), he saw and believed that the world would never again be quite the same and he was right.

JOHN E. REXINE
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JOHN MATTHEWS. *Western Aristocracies and the Imperial Court A.D. 364–425*. New York: Oxford University Press. 1975. Pp. xiv, 427. $31.25.

Using prosopography, John Matthews traces the political history of the Roman West during a crucial period by studying the careers of statesmen and the effect that their affiliations had on politics. The narrative stresses the diverse backgrounds of politicians, the varied roles of the imperial court as an institution, the Christianization of the elite, and the exercise of power by magnates. A short review can merely suggest the more important conclusions.

Senatorial power, enhanced by the structure of late imperial society, was, as always, based on *clientelae* and fluctuated during succeeding reigns. The reign of Valentinian I was characterized by conflict between professional bureaucrats and senators, who were essentially amateurs in government. The latter, led by Ausonius, gained ascendancy beginning with Gratian, the renewed residence of emperors in Italy enhancing the influence of Italians like Symmachus.

The reign of Theodosius I, after Adrianople, brought another faction to power, which was dominated by Spaniards. His solution to the Gothic crisis wins praise from Matthews, qualified by the observation that its implications were disquieting for the future conduct of Roman diplomacy. The author analyzes the evolution of Theodosius into a devout Catholic emperor, whose personal bent made him responsive to Church pressure. Chapters on Christianity and the courts at Constantinople and Milan and on the development of evangelism and heresy among the provincial establishment expand the subject.

The author praises Stilicho as a regent who understood the limitations of imperial power following Theodosius' death. Stilicho's assassination led to unprecedented invasions, from which Roman civilization emerged more localized and, outside the aristocracy, more narrowly based. By A.D. 425, however, stability had been regained in Italy and Gaul, where contemporaries could even contemplate a restoration of their world. By then, however, western senators were clearly a more vital force in effecting that restoration than were formal institutions of Roman government.

This is an interesting synthesis of political, social, and cultural history. Specialists and students will read it with profit because it portrays lucidly and subtly the interplay of forces producing the decline of West Rome. One hopes that the inflated price of the book will not limit its use.

WILLIAM G. SINNIGEN
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JAN CZARNECKI. *The Goths in Ancient Poland: A Study on the Historical Geography of the Oder-Vistula Region during the First Two Centuries of Our Era*. Coral Gables, Fla.: University of Miami Press in cooperation with the American Institute of Polish Culture. 1975. Pp. xiv, 184.

*The Goths in Ancient Poland* presents a revised image of the Goths in the Oder and Vistula region during the first two centuries of our era. It is a careful historical-geographical investigation that should be useful to historians and archeologists. Jan Czarnecki contends that, although incomplete, the contemporary reports by Strabo, Pliny the Elder, Tacitus, and Ptolemy are trustworthy sources that have never been systematically examined. He cross-checked these accounts to reconstruct the earliest presence of the Goths in continental Europe and located Gothic tribal territories by pinpointing the locations of neighboring groups mentioned in each of the contemporary texts.

It is not uncommon to read about the Goths during the period, c. A.D. 200–375, when they built a powerful state in the Black Sea region that posed a threat to the Roman provinces. The less dramatic episodes prior to this—when the Goths were stationed between their homeland in the Öster and Väster Götland and southern coast of the Baltic—are relatively unknown. The author justifiably notes that it is virtually impossible for archeologists to identify the Goths on the basis of artifactual remains, because the Goths never lived on the Baltic coast as a large organized group, and their migration from Sweden was not massive or destructive, but was motivated by economic conditions.

This migration seems to have started in the middle of the first century B.C. and ended during the first century of the Christian era, making it a more gradual and indirect movement than here-

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tofore considered. On the basis of his analysis of primary sources, Czarnecki maintains that these works confirm the presence of the Goths in the Oder region and also in the lower Vistula, according to Ptolemy, who mentioned them among the peoples of European Sarmatia. The author's research further demonstrates that this lower Vistula group of Goths was positioned between the estuary of the River Narew and the Gulf of Danzig, then occupied by the "Venedian" tribes (which, despite the author's opinion that they were Slavic, seems quite doubtful). He asserts that the two groups of Goths were not separate entities, but one: the Vistula Goths came from the Oder region shortly after A.D. 98 where they remained for about seventy years before pushing toward the Black Sea.

This book sheds much light on the location of the Goths in Poland and now calls for the work of archeologists and linguists to make the Gothic issue even more comprehensible.

MARIJA GIMBUTAS
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## MEDIEVAL

JEAN-NOËL BIRABEN. *Les hommes et la peste en France et dans les pays européens et méditerranéens*. Volume 1, *La peste dans l'histoire*. (École des hautes études en sciences sociales, Centre de recherches historiques. Civilisations et Sociétés, number 35.) Paris: Mouton. 1975. Pp. 455. 150 fr.

Plague killed some fifty million Europeans. The size of that figure tends to support the contention of the French social historian Louis Chevalier that the way people die reveals much about the way they lived. Jean-Noël Biraben has proved Chevalier's argument in his important new book on the plague and its impact on France and the Mediterranean world.

Pulling together an almost unbelievable number of primary and secondary sources, Biraben has gone further than any other historian in identifying the presence and spread of this destructive disease. Using a statistical approach, Biraben proves conclusively that after 1350 the disease was endemic to Europe, reaching epidemic proportions periodically. Adroitly using methods known to medical cartography, he shows this particularly in the case of southern England, southern France, and eastern Spain.

Focusing on the plague of 1347–50, Biraben sees the disease following two paths. It leaps from one area to another when it is seaborne, but moves rather slowly overland. Biraben adds to our knowledge in other ways, suggesting that plague was more urban than rural, that it almost always had a seasonal and durational character, and that there was no apparent correlation between famine and the outbreak of plague.

Biraben's study is so solid in most places and is such an outstanding contribution that its shortcomings might be passed over were they not glaring. One is his use of evidence. As a demographer, Biraben relies too heavily on statistical and not enough on symptomatic evidence. One of the few times he uses symptomatic evidence is in his discussion of the plague that touched Europe and Byzantium in the sixth century. The quickness with which death ensued, the high fevers and almost universal attack on the lymph system constitute sure-fire evidence of the plague's presence. But after this, Biraben passes right over symptomatic evidence. The chronicles of the 1340s that discuss the spitting of blood surely indicate the deadly pneumonic form of the disease, but Biraben ignores this evidence. He concentrates on the plague that hit Marseille in 1720, but again fails to see that its extraordinarily contagious character meant that it was again the pneumonic form. Beyond this, Biraben's easy acceptance of Ricardo Jorge's dictum that the microorganism (*Pasteurella pestis*) did not change indicates a lack of bacteriological knowledge, since all bacteria mutate. A second shortcoming is the absence of sociological knowledge. For example, we know from this book that hordes of people died, but except for one study of Lyons, we really do not know which social groups suffered the most.

All of this makes Biraben's book an important building block to further understanding of the plague in Europe, but not the final part of the story.

VINCENT J. KNAPP
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J. J. G. ALEXANDER and M. T. GIBSON, editors. *Medieval Learning and Literature: Essays Presented to Richard William Hunt*. New York: Oxford University Press. 1976. Pp. xii, 454, xxviii. $41.50.

Occasionally a *Festschrift* can serve a wider purpose than that of honoring its recipient. It can indicate directions in which scholarship is moving and bring together work which has some unity of theme and approach but which would otherwise be unlikely to see publication under a single cover. One such notable *Festschrift* was that presented to Sir Maurice Powicke in 1948, *Essays in Medieval Studies*, edited by R. W. Hunt and two collaborators. The importance of that volume is shown by the extraordinarily large number of citations to it in modern works of scholarship.

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Now a volume of studies presented to Richard Hunt seems likely to be as influential as the 1948 collection he edited. The title, *Medieval Learning and Literature,* gives only a partial indication of the focus shared by many of the eighteen essays—the making, transmission, and influence of books and texts, and through them of learning. Hunt has made this subject his own, both in his published works and in his long and celebrated tenure as Keeper of Western Manuscripts at the Bodleian; and yet these essays by his friends and pupils provide vivid illustrations of how much is yet to be known in this basic field of medieval studies.

Three essays in particular deal with topics which the casual student might assume had long ago been definitively studied. Malcolm Parkes shows how the concepts of *Ordinatio* and *Compilatio* affected the development of the book from the twelfth through fourteenth centuries, the physical shape of a book influencing as well as reflecting its content. (Eight well-chosen plates tell much of his story.) Neil Ker's investigation of "The Beginnings of Salisbury Cathedral Library" illuminates, through a masterly scribe-by-scribe analysis, the earliest major library in England to be put together *ab initio* after the Conquest. And Bernhard Bischoff discusses the *Hofbibliothek* of Louis the Pious, one generation removed from Charlemagne's but much less intensively studied.

One other essay of major importance, though not so closely connected to the main theme, is Sir Richard Southern's treatment of the virtual commonplace that Vacarius lectured in Roman law at Oxford in the mid-twelfth century. Southern (who has earlier gone a long way toward disposing of the "school of Chartres") suggests how it came to be assumed in the thirteenth century that Vacarius had been what we would call a university teacher, which in turn implied that the place where he taught was a university, and shows how unlikely this is to have been the case.

In summary, this thick and well-produced volume would more fittingly be included in the reference room of a research library than in the stacks among dozens of little-thumbed *Festschriften.*

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HANS KAMMLER. *Die Feudalmonarchien: Politische und wirtschaftlichsoziale Faktoren ihrer Entwicklung und Funktionsweise.* Cologne: Böhlau Verlag. 1974. Pp. xii, 284. DM 58.

Hans Kammler's book on the political, economic, and social factors underlying the development and functions of the feudal monarchies poignantly illustrates why no one, unless master of sociological and historical methodology as well as of the historical period concerned, should make such an attempt. On its historical side it is completely unoriginal, simply using the historical literature devoted to feudalism, such as works by Marc Bloch, Robert Boutruche, F. L. Ganshof, Otto Brunner, Otto Hintze, Walther Kienast, and Heinrich Mitteis, and reproducing their conceptual and chronological framework. In his frequent comparisons between Western and Japanese feudalism, the author relies on such scholars as K. Asakawa, John W. Hall, and Karl A. Wittfogel. Since these are all experts on various aspects of feudalism, the author makes few errors in his remarks on the historical development of feudalism in Western Europe. But what is the point in repeating what these experts have already written about the causes of feudalism, the influence of the urban and money economy on feudal institutions, and the use of feudalism by rulers of the developing states in Western Europe during the twelfth and thirteenth centuries?

What is even more irritating about this book is its claim to present new insights into feudalism through a sociological explanation of the relevant historical data. Frankly, most of the sociological studies used seem to have little connection with the historical parts of the book. It is pointless, for example, to use Max Weber, because he never understood the functions and principles of western feudalism. But Weber's works did at least show possibilities for wider comprehension of feudal society, which is not the case with most of the other sociological works cited, as can be seen from the following examples. Every European historian, even the non-medievalist, knows that the Carolingians had the twin problems of communication and control, and that these problems were connected with the rise of feudal institutions and the transformation of the Carolingian state. To help our comprehension of these problems we are given two quotations, one from Jay W. Forrester's *Urban Dynamics* (1969): "Feedback loops are the fundamental building blocks of systems," and another from Norbert Wiener: "Communication alone enables a group to think together, to see together, and to act together. All sociology requires the understanding of communication" (p. 114, 96n). But what is the relevance of these sociological pronouncements for the problems of Louis the Pious, Charles the Bald, and Charles the Fat? Further, what can the writings of Thomas Jefferson and Thomas Paine add to a discussion of the failure (*Scheitern*) of the Carolingians?

If these examples fail to convince how ridiculously this book employs sociology in the service of history, let those who remain dubious refer to pages 76–77, 98, and 103–105 where appear dia-

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grams and clusters of figures resembling those in a modern book on molecular biology. To explain them the reviewer has neither the competence nor the stomach. A distorted imagination is needed to see how this book can be of value to either the historian or the sociologist.

BRYCE LYON
*Brown University*

ANTONIA GRANSDEN. *Historical Writing in England, c. 550 to c. 1307*. Ithaca: Cornell University Press. 1974. Pp. xxiv, 610. $37.50.

Those who have felt the need for a survey of historical writing in medieval England will find that Antonia Gransden has provided a very useful volume that will serve as a basic work of reference for many years to come. In twenty-one chapters of unequal length the historiographical landscape is covered from Gildas's *De Excidio Britannia* through the reign of Edward I. Even the Bayeux Tapestry is discussed. Gransden indicates, "This book is a survey of chronicles and biographies written in England to the end of Edward I's reign. It includes works which though not written in England are of primary importance for English history." She might have added that the author of a given work need not have been an "Englishman," whatever that may mean, during the seven and one-half centuries covered by this volume. Although little seems to have escaped the author's definition of historical writing in England c. 550–1307, it might be asked where Scotland, Ireland, and Wales fit within Gransden's conception of "England." Within her perhaps inconsistently applied criteria for selection, it is clear that Old Norse and Celtic works do not receive the attention which they deserve.

Some readers might quibble with Gransden's approach to historical writing, which she considers "pragmatic, not theoretical." For example, chapter 2 is devoted to Bede, but deals almost exclusively with the *Ecclesiastical History*. His *History of the Abbots of Wearmouth and Jarrow* is treated in the chapter on "Anglo-Saxon Sacred Biography and Local History," which is a chronological survey covering about four centuries. In short, is it really "pragmatic" for a historian to divide the intellectual product of a single mind according to literary theories of genre?

The reader will look in vain for a chapter of conclusions and will find that no general thesis informs the work. Perhaps this is as it should be since the great number of writers who lived under diverse circumstances during the long period covered by the book very probably had little in common. Yet, with few exceptions, Gransden avoids treating either the individual authors she considers as intellects or their work as aspects of intellectual history. Rather, she tends toward evaluating each text with regard to its usefulness to the modern scholar who might want to use the evidence it provides for dealing with one or another problem in medieval "English" history. This leads to the extraction of a particular piece from its own time and values so that it might be judged by canons of truthfulness and accuracy developed during the nineteenth and twentieth centuries.

Gransden has provided an excellent index, an inadequate bibliography, and one potentially valuable appendix listing the principal sources in chronological groups. This, however, should have been exhaustive rather than selective. The three other appendixes are more in the nature of scholarly notes. Most of the deficiencies in Gransden's volume could easily be remedied in a second edition. The publisher might be well advised to use *Historical Writing in England* as the first volume in a series of reference works that would cover medieval France, Germany, Spain, and Italy.

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HARRY ROTHWELL, editor. *English Historical Documents*. Volume 3, *1189–1327*. New York: Oxford University Press. 1975. Pp. xxiv, 1032. $63.00.

Harry Rothwell's volume, which completes the medieval part of this series, will be a useful pedagogical resource. It is ironic, given the series' purpose—"to make generally accessible a wide selection of the fundamental sources of English history"—that the price of the book will place it beyond the means of most individuals, not to mention many smaller libraries. Nonetheless, for those who can afford it, there is a lot here.

The book is divided into six main sections which reflect in a crude way the traditional emphases of the curriculum of English medieval history as taught in some British universities. Although the editor notes that "the emphasis in both the teaching and the writing of history has shifted in this century from the political and constitutional . . . to social aspects of the subject" (p. 294), his organization and selection of documents leave no doubt as to his own main interests. Social and economic history occupy a total of 105 pages, intellectual history 114 pages. The Church gets 149 pages, largely devoted to institutional developments. Chronicles of political narrative receive 247 pages, constitutional and legal developments 249 pages, and practical royal administration a further 81 pages.

The translations have been chosen and frequently emended with great care, although no at-

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tempt has been made to standardize or modernize the spellings of proper nouns. The notes are sparse, and such footnotes as appear ordinarily relate to the textual reading or translation adopted by the editor. One might have wished for more in the way of specific introductions to the selections, explanatory footnotes, or at least a glossary to guide students through the numerous technical terms which are sometimes explained in the notes, but usually not. In a few cases the footnotes could be more to the point. For example, the Jews' *rotulum* or "scroll" on which they were to swear was in all probability the Torah (p. 306, 1n); and canon 18 of the Fourth Lateran Council (printed on p. 654) prohibited clerical participation in ordeals, so canon 3 should hardly be construed to mean that accused heretics were to clear themselves "by compurgation or ordeal" (p. 647, 1n). Obvious errors, however, are few.

The editor's general introductory remarks are of uneven quality. At best they are concise and informative, admirably summarizing traditional historiographical controversies such as those over the significance of Edward II's coronation oath (pp. 300–01) or the origins of the open-field system (pp. 783–84). At worst they are banal—"Human societies do not go into a state of suspended animation except in fairy tales" (p. 1)—and repetitious (on pp. 16 and 22 the same anecdote is told of Gregory the Great, nearly verbatim).

The bibliographies pay homage regularly to Stubbs, Maitland, Tout, and Powicke; not surprisingly, they are most helpful in the sections on political narrative and royal government. The others either are extremely short (for example, that on the thought of the period) or degenerate into broadly categorized lists without critical comment ("Land," "People," and "Miscellaneous" on pp. 789–98). They will hardly serve as systematic or comprehensive introductions to the literature for undergraduates.

The verdict, then, is mixed: the translated selections are most welcome, but the introductions and apparatus fall short of expectations.

JOHN S. BECKERMAN
*Yale University*

M. BRETT. *The English Church under Henry I.* (Oxford Historical Monographs.) New York: Oxford University Press, 1975. Pp. xii, 282. $19.50.

The title of Martin Brett's book, *The English Church under Henry I*, is misleading, for it is not so broad a study as the title indicates. As he himself admits, this is not a comprehensive history of the early twelfth-century English Church, but an institutional study of the ecclesiastical hierarchy from pope down to parish clergy. The ranks below the cathedral dignitaries, however, receive little attention. Although he does include one short final chapter on the parish clergy, he ignores completely the monastic clergy; neither does he dwell upon the personalities of the bishops of Henry I, or their spiritual concerns. His chief interest is in the organization of the Church. It is unfortunate that an earlier, less useful study had pre-empted a title more appropriate for Brett's book: Everett U. Crosby's "The Organization of the English Episcopate under Henry I" (*Studies in Medieval and Renaissance History*, 4 [1967], 1–88).

Brett defends the limitation of his study to the English Church under Henry I by stressing its unique character in the early twelfth century: a Church with a long Anglo-Saxon tradition, now acted upon by both the reforming papacy and Norman monarchs. The organization of the work is topical rather than chronological with chapters on the English Church's relations with its neighbors, the other British Churches; on papal jurisdiction in England; on the two archbishops; on the bishops in their dual roles of pastors and pontiffs, and of judges and administrators; on the cathedral chapters and episcopal officers; and on the parish clergy.

A number of useful points are made by Brett, particularly in his consideration of papal jurisdiction in England. His findings force a modification of those set forth by Norman F. Cantor in his *Church, Kingship, and Lay Investiture in England 1089–1135* (1958). Brett points out that the archbishop of Canterbury had as much reason to resist papal claims to authority over the English Church as did the Norman kings, and that the great obstacle to papal jurisdiction was not so much conscious royal policy as "the physical and intellectual distance between England and Rome." Further, he finds the influence of the archbishop in secular affairs steadily declining from the high point reached with the relationship between Lanfranc and William. The chapter on the parish clergy is the shortest and weakest, as Brett himself concedes. He notes that he has had to write his history, as it were, upside down, without uncovering "the critical problem of the relations between the clergy and the bulk of the laity."

Brett's views are sensible, and he achieves his goal of bringing into light "the shape of the superstructure of Henry's church." Yet sometimes an opaque style casts unnecessary shadows. Further, in this time of dying classical studies, is it wise to include so many Latin quotations? Brett insists on quoting in Latin, even when acceptable English versions are available, for example, Marjorie Chibnall's edition of *Ordericus Vitalis*.

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G. L. HARRISS. *King, Parliament, and Public Finance in Medieval England to 1369*. London: Oxford University Press. 1975. Pp. xii, 554. $37.75.

The title of this book is descriptive of the contents, with the emphasis upon the emergence of public finance and its interrelation with the two institutions. Because of its heavily factual content and weighty style, it is more likely to be used in parts than as a whole, but the author has produced a masterly synthesis from a mass of difficult sources. One of the major criticisms that might be leveled at the subdiscipline of administrative history as it has been developed by scholars who have followed in the steps of T. F. Tout is that some particular part of the administration has been examined without reference to the larger society or times of which it was a part. Harriss has overcome this difficulty by blending administrative history with political and constitutional history on the one hand and the history of political and legal theory on the other. For example, the views of Gaines Post in his study of political and legal theory prove especially illuminating in conjunction with the growth of national taxation. The author discusses the concept of "fisc" as the inalienable endowment of the Crown in terms borrowed from political theory in place of the venerable but confusing usage of "royal demesne" more familiar to English medieval history. Although the command of a vast amount of secondary literature is impressive (as might be expected in a work of synthesis), the core of the book, dealing with the financial work of the Exchequer, is new and rests to a large extent upon original manuscripts.

The book has important implications for most of the major themes in English medieval history from the late twelfth century through the first phase of the Hundred Years War. Consider this statement from the concluding chapter: "The emergence of a system of public finance in England, which has been the theme of this volume, was an essential feature of the early nation state." What is outstanding about the treatment is the way in which events, theory, and institutional development are considered together as a dynamic process. For this reason, the author has something to contribute to such old problems as the crisis of 1294–97 and the Ordinances of 1311. Nor is he wedded to one type of explanation. He corrects Eileen Power's economic explanation of the establishment of the right of the Commons to assent to indirect as well as direct taxes in connection with the *maltolt* on wool in the 1340s by examining in detail the political situation that provides a more convincing explanation. His explanation of the process by which the academic doctrine of the *Modus Tenendi Parliamentum* became the consensus of the knights and burgesses in Parliament by 1340 closes the credibility gap that has too long separated scholars who concentrate exclusively on the theory of the growth of Parliament or on the practices of a parliament at work.

The book is narrow neither in its scope (as shown by its title) nor in its conclusions. Undoubtedly, it will become one of the "standard" volumes to place alongside the venerable constitutional history of William Stubbs and the pioneering administrative history of T. F. Tout as a worthy example combining both approaches to English medieval history.

CHARLES R. YOUNG
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GEORGE HOLMES. *The Good Parliament*. New York: Oxford University Press. 1975. Pp. viii, 206. $19.00.

We know considerably more about the Good Parliament of 1376 than about any other English parliament of the Middle Ages. Three detailed independent chronicle sources supplement an unusually complete set of official records. By meticulous use of this information as well as a wealth of manuscript material, George Holmes has created an account of the specific economic, political, and social forces which drove both lords and commons to such a dramatic outburst of anticourt sentiment. As such it is a welcome extension of earlier work by Bayley and Bellamy on the impact of the political environment on parliamentary proceedings and a significant departure from the traditional focus on constitutional development.

Holmes is particularly effective in demonstrating how the royal court alienated each of the politically important groups in English society during the years immediately preceding the parliament. By abandoning its traditional resistance to papal pressure for subsidies the court angered the bishops; by bungling military campaigns in Brittany and by agreeing to a humiliating truce with the French the king's advisers evoked disgust among the lay lords; and by pursuing imprudent financial dealings Latimer and other courtiers embittered the merchants whose complaints were voiced mainly by the knights. These specific grievances, combined with a general antipathy toward the corrupt court, led to the impeachment of several courtiers and, more importantly, to a dramatic refusal by both parliament and convocation to grant any subsidy to the king.

The best section of the book deals with the aftermath of the Good Parliament. Noting the pardoning of condemned courtiers within a few months and the compliance of the next parliament with government wishes, Holmes convincingly attributes the court's rapid recovery to a significant

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shift in policy in the wake of the Good Parliament. By the Hilary Parliament of 1377 the court, now more strongly led by Lancaster, had once again made common cause with most of its former foes. The most dramatic example of this shift was in papal policy, where Lancaster returned to the traditional resistance to papal intervention. Holmes' identification of this reversal of court policy with its patronage of John Wycliffe is of particular interest. Although relatively brief, this book lucidly presents the most complete account of the Good Parliament that we are likely to have.

THOMAS CALLAHAN, JR.
*Rider College*

LOUISA DESAUSSURE DULS. *Richard II in the Early Chronicles*. (Studies in English Literature, volume 79.) The Hague: Mouton. 1975. Pp. 274. $16.95.

Although intended for scholars of Renaissance literature, this study is of great value to historians interested in the reign of Richard II. Louisa Desaussure Duls delineates three traditions of Richard II in the chronicles: the Lancastrian view of a "wicked" tyrant; the French view of a "martyr" king; and the Yorkist view of Richard as the victim of an ambitious pretender. Then the development of these traditions is traced by analyzing chroniclers' accounts of Richard's behavior during six crucial episodes in his career. Here, for the first time, the historian can find a collation of chroniclers' views—whatever their prejudices and biased judgments—on, for instance, Richard's last days. If they fail to concur on all points, some chronicles at least serve as correctives of the depiction of Richard II after 1397 as a weak-kneed, neurotic figure which has become so fashionable in recent years. Jean Creton's *Metrical History*, written from the perspective of an eyewitness, tells us that at Conway Castle in 1399 Richard was "pale with anger," and that he promised to even the score with his enemies: "Some of them I will flay alive." As Duls points out, even some Lancastrian chroniclers do not accept the Saint Albans account that has Richard resigning his crown *"vultu hilari."* Adam of Usk suggests compulsion at Conway, and the *Vita Ricardi* omits altogether the promise to resign.

Here and there one finds minor flaws. The Saint Albans *Chronicon Angliae* can safely be attributed to Walsingham. The *Vita Ricardi* proves to be, upon close reading, a composite, and its dual authorship reflects alternating views of Richard. But these points detract little from this estimable work; it is required reading for an appreciation of Richard's reception in his own time.

GEORGE B. STOW, JR.
*La Salle College*

A. W. B. SIMPSON. *A History of the Common Law of Contract: The Rise of the Action of Assumpsit*. New York: Oxford University Press. 1975. Pp. xlv, 646. $35.00.

A. W. B. Simpson follows the history of contract law in the English common-law courts from the early fourteenth century to the Statute of Frauds of 1677. His is a labor of enormous scholarship and incisive analysis, a history of legal doctrine that rises to a history of ideas, all presented gracefully and often with striking human immediacy. The route that the author pursues is the familiar one, through the medieval actions of covenant, debt, and detinue, to his principal theme in the rise of *assumpsit*. But he illumines so much of the way that has been dark before: the law of common callings in the fifteenth century, the wearing away in the fifteenth and early sixteenth centuries of the rule that *assumpsit* did not lie for nonfeasance, the background in penitential manuals of Christopher St. Germain's thought on the moral status of promises, the particulars of Slade's Case of 1602 (which, it turns out, was not in *indebitatus assumpsit* at all), and much else. The best of all is a long and careful new study of the early doctrine of consideration.

I am not satisfied with the account of the meaning of *assumpsit* in the fourteenth-century cases. There is no time to argue the matter here, and if it were argued in full perhaps Simpson would prove to be right. Otherwise, I find nothing whatever to grumble at except a lot of details. No adequate final check was made of the text, and consequently it contains dozens of typographical and other incidental errors, some of which will cause the reader real difficulty (for example on pp. 21, 85, 472). Latin passages and translations from Latin and French swarm with embarrassing mistakes; Simpson, one gathers, is not fully at home with the languages. It is unsettling to realize this, for the study is largely based on Latin and French materials. But I have noticed only two places where this sort of error spoils the argument (p. 338, where *duist estre*, "supposedly was," is rendered "ought to have been," and p. 400, where *futurorum* is taken to mean "of the dead"), and even here none of the author's main theses is damaged. Simpson has built his study on so many cases that particular misunderstandings are not likely to mislead him. Still we lament such faults in our students, and it is sad to find them in our teacher.

DONALD W. SUTHERLAND
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NICHOLAS H. STENECK. *Science and Creation in the Middle Ages: Henry of Langenstein (d. 1397) on Genesis*.

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Notre Dame: University of Notre Dame Press. 1976. Pp. xiv, 213. $14.95.

Despite the existence of specialized studies of some aspects of medieval science, few overviews accessible to medievalists in general and to historians of science of other periods have appeared. Steneck has attempted to fill this gap by means of an exploration of the ideas on science and nature in Henry of Langenstein's *Lectures on Genesis*. Limiting himself to a single work of one figure, Steneck abandons any analysis of chronological development and uses only minimally the more technical scientific treatises usually examined. The value of interpretations based on a purely synchronic study will depend finally on how "representative" a figure Henry turns out to be.

That medieval science did not exclusively adhere to religious dogma and Aristotelian orthodoxy has been established by recent scholarship through studies of optics, quantification of acceleration, heat transfer, and compound medicines, practical chemistry, Archimedean mathematical physics, and the logic, arithmetic, and geometry of Euclid in the Middle Ages. Some of this research challenges the traditional view of the differences between medieval and modern science. Steneck argues that continuity can be found not in specific details of scientific problems or their solutions, but in the continued advocacy of the world as a machine connected by mechanical causes. In *Science and Creation* we find no diagrams, mathematical proofs, or studies of motion, but rather the descriptive and discursive natural history of creation—many of the same aspects of the medieval world-picture that C. S. Lewis evoked from literary sources in *The Discarded Image*. For Lewis's clever sampling of poetry, Steneck has substituted a careful, but never plodding, reading of an extensive work by a major scientist and science teacher in philosophic terms.

Henry of Langenstein (or of Hesse) is an apt choice for such an exposition. Not the most original of fourteenth-century thinkers, he nonetheless enjoyed some intellectual prominence while sharing typical career experiences as arts professor (at Paris), court intellectual (for Charles V of France), ecclesiastical politician (in the Conciliar movement), university administrator (at Vienna), and teacher-theologian (at a Cistercian abbey and at Vienna). He wrote on optics, astronomy, some books of the Aristotelian corpus, astrology, and the Bible.

For understanding natural philosophy of the period, this Genesis-commentary serves well, for all of the natural (and supernatural) world is included in a scholastic reading of the days of creation, and the *Lectures* form a virtual encyclopedia for their age. (Although encyclopedias were popular compositions in the thirteenth century, and earlier and later, the fourteenth century ignored this genre.) Because the text of the as yet unedited *Lectures* would occupy two or three thousand printed pages, Steneck has extracted the scientific parts from the manuscripts in a successful paraphrase. The theme of Henry's work—and, Steneck argues, the theme of scholastic science in general—is the demonstration of the (divine) order of the natural world, a world integrated for Henry by chains of mechanical causes. Steneck correctly emphasizes this "Augustinian" motive for the popular and academic study of nature—to admire God's handiwork, to perceive and demonstrate its order and perfection. But having noted that science was then "an intensely religious activity" (pp. 15 and 142), he fails to deal with the interesting historical questions this fact raises. The same *ratio* underlies neo-Platonic concern for nature in the Renaissance (think of Ficino, Pico, and John Dee), the new science of the seventeenth century (Kepler, Boyle, and many others), and the natural theology of the eighteenth century. To recognize this as an approach to nature without specifying the exact character of the "religious" motive itself and of its metamorphoses through time tells us little about either the continuities or the discontinuities between medieval and modern times.

The small interpretive weaknesses of this synchronic treatment are offset by its readability, clear focus, and pedagogical utility. Specialists will mine *Science and Creation* for facts and references, perhaps ignoring or disputing the interpretations. At the same time they will require it of their graduate students and recommend it to their colleagues, for no other book describes so well this range of ideas in the medieval experience of the world of nature.

BERT HANSEN
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SARAH RUBIN BLANSHEI. *Perugia, 1260–1340: Conflict and Change in a Medieval Italian Urban Society*. (Transactions of the American Philosophical Society, New Series, number 66, part 2.) Philadelphia: the Society, 1976. Pp. 128. $8.50.

This monograph examines the social structure of Perugia in conjunction with its economics and politics and its domination over its *contado* and the localities beyond the *contado* from about the end of the consular regime in the last decades of the twelfth century to the time of the Great Plague in the mid-fourteenth century. Within this period, it emphasizes the years 1260–1340 about which a

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number of official documents yield particularly valuable informaton.

Sarah Rubin Blanshei introduces her study with an assessment of earlier interpretations of communal history. She has mined tax records and private documents with devotion and intensity, and has elucidated the factors that shaped Perugian history at the time. Perugia expanded economically and demographically in this period, and, contrary to past beliefs, fairly dependable conclusions about its population can be made if its topography and other factors, as well as official records, are taken into account. Blanshei ventures to calculate the city's population as 30,757 in 1285, and that of the *contado* as 73,166 at about the same time. In exemplary analyses, she shows that Perugia drew people from the *contado* and beyond from many levels of society but particularly from the *nullatenenti*, the "non-possessors."

The thirteenth- and fourteenth-century constitutions leave no doubt that there were conflicts of interest between the *magnati*, the old landed and knightly class, and the *popolo*, the wealthy and middling non-nobles, the artisans, and the workers. Salvemini and others have demonstrated this division for Florence, but it may have been less sharply drawn than they indicated. In Perugia, the oldest families lived in parishes at the center and controlled politics until the *popolo* (less sharply divided between *popolo grasso* and *minuto* than in Florence) moved into power. The *capitano del popolo* appeared first in 1255, and the *iudex iustitie*, an officer peculiar to the *popolo* of Perugia, in 1293.

Blanshei shows that it is possible to uncover the forces of social and political change in a commune, and this encourages further inquiry. Russell, in *Medieval Cities and Their Regions*, has raised the question of metropolitan regions, suggesting that Rome was within the metropolitan region of Florence. Alice Walsh, in a study of the papal administration of the duchy of Spoleto has shown that Perugia was Spoleto's great rival in the first half of the fourteenth century. Blanshei attributes the commercial growth of Perugia to its favorable location on the Via Amerina between the Viae Flaminia and Cassia and the Lake Trasimeno fisheries. She shows how Perugia drew population from the *contado* and, unlike other towns in the area, from more distant places. Was Perugia the most important town in this area economically, and could it be the center of a metropolitan region? Here further probing may yield insights that are particularly fruitful also for the present.

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ALPHONS LHOTSKY. *Aus dem Nachlass*. Edited by HANS WAGNER and HEINRICH KOLLER. (Alphons Lhotsky, Aufsätze und Vorträge, number 5.) Munich: R. Oldenbourg Verlag. 1976. Pp. 408. DM 49.

Until his death in 1968, Alphons Lhotsky was the foremost authority on the history of the late Middle Ages in the Austrian lands and, as such, a powerful influence on Austrian historical thinking in the period since the Second World War. His friends and admirers have honored him with a memorial publication of some of his lectures and essays. The volume under review, the fifth and last in the series, consists of eighteen pieces the majority of which were lectures given before such varied audiences as the Twelfth International Congress of Historical Sciences, groups of school teachers, and meetings for continuing education.

All but three of the contributions date from the last nine years of Lhotsky's life and are therefore the result of his mature scholarship and consideration. Despite the variety of topics, the thought and personality of the author provide a unifying factor. Lhotsky's passionate love for Austria shows through on almost every page. He conceived of Austria as the lands left to the republic after 1918. It was his ambition to restore the historical ties between the Second Republic and the age of the Babenberg and Habsburg rulers—a bond that was deliberately severed by the politicians of the interwar period—and to instill an awareness of this historical continuity in the popular consciousness. To this end he labored tirelessly in the continuing education of teachers and others.

Lhotsky's Austro-centricity leads, however, to some curious conclusions. In discussing the problem of periodization of the Late Middle Ages, Lhotsky tentatively suggests that the age was ushered in in 1282 when Rudolf of Habsburg reached the zenith of his power at the Diet of Augsburg and ended in 1522 when Charles V granted his brother Ferdinand rule over the Austrian lands. Elsewhere, he is very critical of Charles' Austrian policy, which he thought threatened to break up these very same Habsburg lands. Nor does Lhotsky apparently see any clear division between the age of the Baroque—one of Austria's two cultural epochs par excellence—and that of the Enlightenment, unless it be the replacement of Italian cultural influence by the more baneful French during the Diplomatic Revolution of 1756. Lhotsky also makes some pertinent comments on the present-day crisis of the historical profession, in particular the failure of the academic historian to reach a wider public.

HANNS GROSS
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JOHN MONFASANI. *George of Trebizond: A Biography and a Study of His Rhetoric and Logic*. (Columbia

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Studies in the Classical Tradition, number 1.) Leiden: E. J. Brill. 1976. Pp. xi, 414. 96 gls.

John Monfasani has written an important and well-executed book illuminating the career and writings of George of Trebizond. It is an important contribution to our knowledge of fifteenth-century thought as well as a carefully documented and reasoned account of George's relationships with many of the famous and powerful people of his day. Particularly good is the study of Trebizond's rhetoric, its relationship to his logic, and its subsequent influence, especially on the northern humanists.

The author believes, correctly I think, that he has found the key to George's thought and sometimes bizarre actions in his growing conviction, fueled by the advance of the Turks and the eventual fall of Constantinople, that the end of the world was near but that some of the worst miseries of the last days might be averted if the sultan could be converted to Christianity. Exemplifying a perversity of human nature which is with us yet, George saw all of his personal enemies as agents of cosmic evil and himself as the prophet who could save the Church and humanity from great suffering.

Monfasani, unlike many who write on the humanists, has a firm grasp of the medieval traditions within which the writers of the quattrocento operated, and this, along with his competence in using difficult manuscript material, is the great strength of the book. It is a credit both to the author and to his teachers, P. O. Kristeller and Eugene F. Rice, Jr.

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JADRAN FERLUGA *et al.*, editors. *Glossar zur frühmittelalterlichen Geschichte im östlichen Europa.* Series A, *Lateinische Namen bis 900:* Part 1, *Einleitung, Abkürzungen und Siglenverzeichnisse;* Part 2, *Quellensiglenverzeichnis, Aba-Alania;* Part 3, *Alanorum Montes-Antes;* Part 4, *Anthaib-Attila;* Part 5, *Atto-Avari.* Series B, *Griechische Namen bis 1025:* Part 1, *Einleitung, Abkürzungen und Literatursiglenverzeichnis.* Wiesbaden: Franz Steiner Verlag. 1973–1975. Pp. lxiv; lxv-lxxiii; 50; 51–110, 111–170; 171–230; lxiv. DM 32 each.

This extremely important collection of documents from the early history of the Slavic peoples is an effort to do for Eastern Europe what the *Monumenta Germaniae Historica* has done for the lands of the Holy Roman Empire. Begun over twenty years ago, this ambitious undertaking, funded by the West German government, produced its first published volumes only in 1973 and is still far from completion. Unlike the *MGH*, however, the *Glossar* is not devoted to publication of entire documents; nor is it simply a glossary, despite its title, but rather a collection of excerpts from written sources organized thematically around key terms. Locating materials on the early Slavs has always been a major problem for historians and philologists, because information is either scattered in records from the German and the Byzantine empires or else contained in East European source books that focus narrowly on one country, province, or institution. For the first time, this much-needed *Glossar* gathers into one place, in an easily used format, thousands of diverse source materials.

In keeping with the general theme of the project, the editors include all materials with a bearing on the arrival, settlement, and early institutions of the Slavs, which means effectively any activity within the large area bounded roughly by the Elbe and the Volga rivers and the Baltic and the Black seas. Spanning a period from the early fourth century to about 1250, these sources are divided into three language groups—Latin, Greek, and Slavic (corresponding to Series A, B, and C of the *Glossar*)—and include inscriptions, seals, and maps as well as more commonly used writings such as chronicles, charters, letters, saints' lives, liturgical works, and military treatises. The key terms refer to tribes and persons, both Slavic and non-Slavic, and to places, topographical features, and aspects of Christianity and heathen cults. Alternate forms of the names, along with broader tribal and geographical associations, are meticulously cross-referenced. Additionally, each entry provides a brief identification of the term and a few titles of secondary works that supply particularly helpful notes and bibliographies.

The excerpts, ranging from just a few words to several pages, have not been severely abridged and for many purposes can be used without reference to the complete documents. Otherwise, the *Glossar* is tremendously helpful for lessening the amount of preliminary sifting. Even more important, however, it revives little-used sources and puts into better perspective some readily available materials that have long been given greater weight than they deserve.

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## MODERN EUROPE

THEODORE K. RABB. *The Struggle for Stability in Early Modern Europe.* New York: Oxford University Press. 1975. Pp. xii, 171. $8.95.

FRANK E. HUGGETT. *The Land Question and European Society.* (Library of European Civilization.) London: Thames and Hudson; distributed by Neale Watson Academic Publications, New York. 1975. Pp. 179. $9.95.

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VINCENT J. KNAPP. *Europe in the Era of Social Transformation: 1700–Present.* Englewood Cliffs, N.J.: Prentice-Hall. 1976. Pp. xvii, 253. Cloth $10.95, paper $6.95.

These authors all ask the reader to rethink the patterns into which periods of recent European history have been ordered by other historians. Theodore Rabb's reinterpretative essay of the seventeenth-century crisis suggests a new "framework" for future discussions; Frank Huggett's short work on the land question seeks to move to the forefront of history "the country-dwellers who derived their livelihood from the land" (p. 6); and Vincent Knapp's general account reviews European social and economic change during the last three centuries in terms of what he calls "the new social history." All three of the works were written with students in mind and all contain purposely general bibliographies. Only Rabb's and Huggett's books are illustrated: the former because perception as expressed in painting is part of the argument, the latter presumably because the series in which it appears is known for its liberal use of excellent and frequently rare illustrations.

Each of the authors argues strongly for his reinterpretation, but only Rabb is convincing. He reviews the existing "crisis" literature and then argues that this word should be used in its medical meaning, namely, as a period of disturbance followed by a resolution. The seventeenth-century crisis, then, covered a long period starting "at some point in the half century around 1500" (p. 71). It concerned the balance between central and regional authority, and the struggle continued until the mid-seventeenth century. At no point during these decades did the possibility of violence recede: when actual fighting was not going on the threat of conflict was never far away. There was nothing that could be called a turning point, and the crisis faded as "the politically active forces throughout Europe" achieved by the early eighteenth century the "perception" that the old issues were no longer worth fighting over. The "crucial discontinuity was the result, not of a necessary acceptance of irrefutable solutions, but rather of a willingness to stop pressing dissensions to their logical conclusion" (p. 72). The change in "perception" was contributed to "decisively" "by the revulsion against the brutal excesses of the Thirty Years' War" (p. 118). Rabb includes paintings from the prewar and the postwar periods to show this very clearly: there is a shift from the exaltation of warriors to representations of the horrors of warfare. (The changing perceptions are also recorded in the writings of political theorists such as Locke and philosophers such as Pascal.)

In a disappointingly short concluding chapter the author argues that we have concentrated on "crises" at the risk of distorting the past and that "periods of quieting down may be just as important, just as full of watersheds, as periods of rising up." Historians, "like the psychoanalysts, . . . are poorly equipped to study integration, stability, and the forces that produce them" (p. 147). We must re-equip ourselves to study stability so that "we will not have to resort to words like 'crisis' to draw attention to a period" (p. 151).

Frank Huggett's work on the European land question is a short and incisive review of the problem from the seventeenth century onward. I do not think that one still needs to argue, as the author does throughout the book, that the subject is of major importance and that there was more to history than the movements and beliefs of the "elites." Just as Rabb argues for a focus on the mechanisms of achieving stability, Huggett is concerned that our perception of land and peasants not operate in terms of sharp turning points. Thus he sees the English agricultural revolution as "complex, gradual, and disparate, with its origins stretching back well into the seventeenth century, and in some cases into the previous century" (pp. 65–66). Pre-eighteenth century peasants were not necessarily a brake on agricultural progress, for "it is now clear that many of them played a major part in improving the land and possibly, though this is much less certain, in introducing some of the innovations where they had the opportunity to do so" (p. 52). Correspondingly, at least for England, the role of the great agricultural innovators such as Tull and Bakewell is deemphasized by the author, and the changes that eventually transformed the English countryside are shown to have been accumulative. The enclosure acts finally "tamed" a countryside in which transformations had been going on for quite a long time.

The English agricultural revolution could not be transplanted easily to the Continent, for reasons that had to do with relatively little innovation in the past, with social structures that limited the movement of great masses of people, and with the varied natural conditions from country to country. Though no actual transformation of continental agriculture took place in the eighteenth century a significant change in thinking was accomplished: "a much greater consciousness of the connections between agricultural progress and the structure of society as a whole, a widespread rejection of serfdom, and a greater awareness of the possibilities of commercial farming" (p. 114). Russia alone stood as an exception to this change in consciousness, and eventually paid the price for warding off agricultural modernity.

Both Rabb's and Huggett's works accomplish their goal of reorienting the reader to new possi-

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bilities, Huggett perhaps less so since his point has already been made by others. Vincent J. Knapp's book, however, is extremely frustrating, especially since it is described as the "new social history" (p. xi). This work contains many useful and assiduously collected data, and the sections on diet and standard of living (his main concerns) are done skillfully. But the work does not convince the reader that "the new social history" has evolved good organizing concepts for the period the author describes. Such organizing concepts being absent, the fallback position is always "classes," and Knapp is no more successful with this notion than are other historians relying on similar organization. Thus one reads of the *ancien régime* as a "static society" (p. xiv), of the "rise" and "formation" of classes, the "rise of the masses," the "collapse of the artisan class" and so forth. I do not believe that the new social history consists of the study of the "way of life . . . of the many through the vehicle of social classes" (p. xii), or that social structure at any point in the European past is to be understood solely as class structure, with classes pictured as entities that rise, fall, and challenge each other. Space does not allow a counterargument to be developed fully, but the beginnings of such an argument are already stated in the works of Peter Laslett, Michael Anderson, Pierre Goubert, and others. Without their more subtle understanding of structure, the "new social history" as exemplified in Vincent J. Knapp's book remains the dreary procession of reified aggregates from which the best recent work has been trying to escape.

ANDREJS PLAKANS
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HERMANN KELLENBENZ. *The Rise of the European Economy: An Economic History of Continental Europe from the Fifteenth to the Eighteenth Century*. Revised and edited by GERHARD BENECKE. New York: Holmes and Meier. 1976. Pp. xi, 354. $15.00.

Hermann Kellenbenz is well known for his works on industry, technology, and international trade. Here he attempts a synthesis of economic development in a vast area ranging from France to Russia and from Switzerland to Scandinavia. For the agrarian economy, he presents a complex survey of land use and tenure, crops and agricultural techniques, and stockraising. He effectively traces the erosion of peasant freedoms in diverse areas where market demand (largely from the west), land-to-labor ratio, and local political structure allowed. The analysis of industry, trade, and commerce displays his vast knowledge and understanding of regional diversity. The best discussions deal with mercantilism, the organization of labor and production, entrepreneurs, trade, transport, and the ebb and flow of production in mining, metallurgy, and textiles.

Since economic changes in continental Europe often occurred in response to external demand for foodstuffs and industrial raw materials, it is disappointing that the book's most serious weaknesses concern the broader European economy. Kellenbenz ignores the subtleties of French scholarship and repeatedly treats France in a cursory fashion as an undifferentiated unit. He is not comfortable with general theories of development and uses the two time spans structuring his book, 1500–1630 and 1630–1750, as little more than literary conventions. Major areas of scholarly controversy are ignored (the Weber thesis), excluded (industrial growth in the seventeenth century and the role of the West in retarding continental industry), or discussed so carelessly that they confuse the student and annoy the specialist (the sixteenth-century inflation).

Kellenbenz is least convincing on population and makes few attempts to relate western European population and demand structure to changing agrarian organization, export, and industry in continental Europe. Furthermore, he seems to misunderstand basic demographic measures and follows other German historians in underestimating the value of parish registers, even after twenty-five years of work by French and English scholars have proven their worth. This is tragic, since the voluminous German registers remain largely unworked by modern methods.

Minor factual errors, such as confusing Portuguese for Spaniards and Genoa for Geneva suggest that Kellenbenz should have edited the work himself. There is an adequate bibliography and index, but no notes and no maps. In sum, the book can be useful for reference and for fine summaries of certain topics. It is not a satisfying monograph, nor a worthy addition to the author's distinguished bibliography.

CARLA RAHN PHILLIPS
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WILLIAM N. PARKER and ERIC L. JONES, editors. *European Peasants and Their Markets: Essays in Agrarian Economic History*. Princeton: Princeton University Press. 1976. Pp. viii, 366. $16.50.

These eight articles by rising young economists have been collected and edited by William N. Parker, Professor of Economics at Yale University, and Eric L. Jones, Professor of Economics at Northwestern University. The papers themselves were initially prepared under a grant from the National Science Foundation and discussed and revised in a seminar held at Yale University in

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April 1972, with support from the Council on Western European Studies there.

The collected articles, like so many joint projects, both fail to fulfill their promise in some respects and exceed expectations in others. So far as the promise is concerned, the collection can hardly be said to deal with the broad spectrum of European peasants in either space or time (since concentration is on the peasants of northwestern Europe between the sixteenth and twentieth centuries), and "peasants and their markets" is much too narrow for the broader implications of most of the papers. Nonetheless, this is an extremely interesting collection. Parker's introductory essay ably sets the scene and notes the relevance of the principal theme of each article to the aims of the project as a whole. Jones' afterword assesses the implications of the papers for the current state of the literature on the subject and enumerates the many aspects of agrarian history that have yet to be covered before an integrated agrarian history can be written.

Most of the contributors to this volume are economists first and historians second and their efforts are concerned with a merging of the descriptive literary techniques of the historian with the somewhat more vigorous analysis associated with quantification and microeconomics. The results are remarkably attractive, for most of the authors succeed in utilizing the methods of quantification without subverting conclusions to technique. The essays in the first sections of the book (sections entitled "Communal Agriculture in the Village" and "Private Property and Enclosure") are in general more interpretative and descriptive, and deal with broader subjects and conclusions than those of the last two sections (entitled "Peasants and Industrialization" and "Agriculture in the World Economy"), which tend to be case studies of individual agricultural areas in a limited period of time.

In the first section, Richard C. Hoffman writes an essay on the "Medieval Origins of the Common Fields," which is interesting for its emphasis on the late twelfth and thirteenth centuries as the period most important in the development of communal regulation of the open fields. The article concludes with a survey of the regional literature on the subject, concentrating on the British Isles, the Low Countries, France, Central Europe, and Northern and Eastern Europe. "The Persistence of English Common Fields" and "The Economics of Enclosure: A Market Analysis" by Donald N. McCloskey tackle the subject of enclosure in England, redirecting attention to the enclosures of the sixteenth century and especially calling attention to the "voluntary" enclosures that preceded the enclosures accomplished by parliamentary private acts in the nineteenth century. "Enclosures and Depopulation: A Marxian Analysis" by Jon S. Cohen and Martin L. Weitzman approaches the problem of enclosure from the standpoint of its relationship to labor supply both before and after enclosure took place.

In "Agriculture and Peasant Industry in Eighteenth-Century Flanders" Franklin F. Mendels assesses the relationship between demographic patterns, increased output per acre in agriculture, and the growth or decline of peasant industries, especially the linen-producing industry. Mendels concludes, "I have shown that in rural-industrial areas, improvement in the relative price of linen produced surges in the number of marriages. Rural industry itself thus helped to accelerate the rate of population growth. It not only permitted population growth, but actively promoted it. The role of cottage industry was therefore perverse in the sense that it perpetuated the dismal pressures that had first induced its penetration into the countryside. As long as an outlet was readily found for the output of the cottage industry, this dismal high-pressure equilibrium remained feasible. It was destroyed in the nineteenth century, when competition from machine-made yarn and cloth and from the new urban cotton industry threw the Flemish rural economy and society into a dreadful crisis" (p. 203).

The other essays deal with more limited problems: "Peasant Demand Patterns and Economic Development: Friesland, 1550–1750" by Jan de Vries, "Organization and Change in Productivity in Eastern Prussia" by Robert A. Dickler, and "Scale and Organization in French Farming, 1840–1880" by George W. Grantham. All, however, indicate a real awareness on the part of their authors of the importance of the larger problems and the way in which their own particular studies will ultimately contribute to a deeper understanding of agrarian history.

K. F. DREW
*Rice University*

WILHELM ABEL. *Massenarmut und Hungerkrisen im vorindustriellen Europa: Versuch einer Synopsis*. Hamburg: Verlag Paul Parey. 1974. Pp. 427. DM 85.

Well-known for his studies on prices, wages, and agriculture, Wilhelm Abel now presents a far-reaching interpretation of European agriculture from the sixteenth to the mid-nineteenth centuries. As the title suggests, the author sees this era as one of continual fight against food shortage and hunger, one in which grain production and prices played a central role. He emphasizes the concept of recurring crises, long a favorite of political historians, as the linking factor in his agricultural-eco-

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nomic interpretation. But even with the emphasis on *Massenarmut und Hungerkrisen*, this is not a strict Malthusian approach, for Abel also recognizes the importance in shaping history of environmental and biological elements such as weather cycles and horticultural changes.

The 350 years covered are sub-divided into two periods. The first—the sixteenth and seventeenth centuries—is seen as the onset of the "pauperization" of large segments of the European populace, in the wake of earlier sixteenth-century increases in prices and population. This first period reached a nadir midway through the seventeenth century. But toward the end of the century, agricultural and technological advances along with the developing role of central governmental institutions, began to make inroads in the crisis pattern, especially in the West. Gradually, in the eighteenth and nineteenth centuries, the second sub-period, man became the master of his environment until by the mid-nineteenth century, hunger crises of the old type had virtually been eliminated from Western and Central Europe. Abel's is a deterministic approach only in overview; the road to freedom from starvation was not a straight line of progress, but one marked by recurring famines, such as those in 1570–71, 1622, the 1640s, 1770, 1817, and 1846.

If the book has a weakness, it may be that Abel has placed too much reliance on prices and grain production in his interpretation and not enough on other possible causative elements. Demographic movements in particular and their role in bringing about mortality crises are not discussed in sufficient detail. But this is of minor consequence when measured against the book's overall merits. I cannot do it justice in such a short notice. Although the author graciously and correctly praises local studies for providing the groundwork for studies such as this, his book confirms the importance of broad interpretive history. Specialists will find in it a wealth of new information and accessible data, as well as a challenging reinterpretation of early modern Europe. Non-specialists may find that this is the single best book from which to gain an understanding of the rural economy of this crucial era. We can only hope that it will soon appear in translation so that it may reach the broadest possible audience.

ROBERT GOTTFRIED
*Rutgers University*

ROBERT S. KINSMAN, editor. *The Darker Vision of the Renaissance: Beyond the Fields of Reason*. (UCLA Center for Medieval and Renaissance Studies, number 6.) Berkeley and Los Angeles: University of California Press. 1975. Pp. 320. $14.00.

In his introduction, Robert S. Kinsman sets the stage for this collection of nine essays and tries to disarm potential critics of two methodological problems: the old bugaboo of periodization and the fluctuating definitions of the irrational used by individual authors—including "foolishness," "lunacy," "non-rational," and "opposed-to-perfection."

Two of the more interesting pieces in this somewhat uneven collection include Lynn White's "Death and the Devil" and Paul Sellin's "The Hidden God: Reformation Awe in Renaissance Literature." For White the Renaissance was the most psychically troubled era in European history as indicated by increases in heresy and witchcraft persecutions and especially by the fascination with death. The basic explanation for these troubled attitudes is the rapid, abrupt changes in politics, culture, and religious ideas which accelerated after 1200. The crisis was resolved only in the seventeenth century when people on a mass scale began to recognize the advantageous possibilities that change brought to their own lives.

Sellin focuses on the "irrational" element of predestination in the theologies of Luther and Calvin and posits its acceptance in the minds of English theater-goers to explain certain characteristics of Elizabethan and Jacobean literature, seen most notably in dramas such as Marlowe's *Doctor Faustus*. The argument about Luther would have been more persuasive had Sellin not limited himself to citations from the specialized context of *Bondage of the Will*. Perhaps his analysis might not have been so clearcut had he treated a more popular and more widely read work such as *Freedom of a Christian* in which Luther emphasized trust and freedom in God as opposed to our separation from Him.

The collection indicates the fruitfulness of an investigation of the irrational in different fields and may yet lead to the "history of hysteria" that White augurs in his essay.

JAMES E. BULLARD
*Stanford University*

MAURICE BOSSARD and LOUIS JUNOD, editors. *Chroniqueurs du XVI^e siècle: Bonivard, Pierrefleur, Jeanne de Jussie, Fromment*. (La "Bibliothèque romande.") Lausanne: Bibliothèque romande. 1974. Pp. 276.

MARCELLA GRENDLER. *The "Trattato Politico-Morale" of Giovanni Cavalcanti (1381–c. 1451): A Critical Edition and Interpretation*. (Travaux d'Humanisme et Renaissance, number 135.) Geneva: Librairie Droz. 1973. Pp. 251.

These two works provide sources for the social and political history of the age of Renaissance and Reformation. Little need be said about *Chroniqueurs*

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*du XVI^e siècle*, in which generally hitherto published accounts of the Swiss Reformation are extracted and made available for a larger public. This beautiful volume is nicely balanced between partisans, two Protestant and two Catholic—on the one hand, for example, a compatriot of the fiery Guillaume Farel, a certain Antoine Fromment, who married a former abbess, and on the other, the equally outspoken Jeanne de Jussie, who recounts how in 1535 her convent at Geneva defied liberation *en masse* and sought refuge in Annecy. Such accounts as the latter can serve to break down overworked stereotypes.

The volume of Marcella T. Grendler rates particular comment: it is the first publication of the substance of a not very accessible manuscript text, and it is prefaced with an extensive review of the setting of the text and an interpretation of its contents. She characterizes the work as "a classic profile of a political outsider whose judgment was seriously compromised by personal considerations" (p. 89). The author of the text is not himself the stereotypical Renaissance man. His family was not rising but had fallen upon bad days; he even served some ten years in prison for his debts (which provided time for his historical writings), and unlike the insatiable *uomo universale*, he lamented, with redeeming candor: "I am 46 years old, and I don't know how to do anything." Apart from the friend of Dante, Guido Cavalcanti, the rest of Giovanni's kinsmen in the *Commedia* were relegated to the *Inferno*.

I am occasionally afraid that Grendler would do the same to her subject. Let me first make clear that my concern is not her scholarship, with which I am most impressed. Her industry in paleography and judiciousness in dating a text (*terminus non nante quem* and *terminus non post quem*), especially the former, fill me with envy, as does generally her command of bibliography. She is also expert in running down obscure references. Any exceptions I would take to the work are not in the area of scholarship (though it is tinged, ironically, by too many typographical errors—I counted at least eight in some seventy-five pages) but in that of judgment or opinion. I do not see from the evidence at hand that a moralist like Cavalcanti was really "a would-be Machiavelli" (p. 14), especially in light of J. H. Hexter's recent work in this area. I suspect that the antipathy of the humanists to Scholasticism can be overdrawn, especially considering Paul Oskar Kristeller's *Le thomisme et la pensée italienne de la Renaissance* (1967), which is not in the bibliography, though Lynn Thorndike's old history of magic and science somehow is. It may be only that I am too cynical in detecting a note of ingenuousness in statements like the following: "The close relationship between ethics and politics appealed to Florentines accustomed to a life of business activity mingled with government service and intellectual achievement" (p.35). My prejudice is that the author is a little too categorical on the general subject of civic humanism. Her repeated sense that Cavalcanti's pessimism makes him an anachronism presumes that the *quattrocento* was really a golden age, a matter which I guess expresses our fundamental difference. After all, Cavalcanti may not be so much "a would-be Machiavelli" as a connecting link to the "*poligrafi*" of the *cinquecento*, about whom a certain Paul Grendler has written so admirably. But this is not to sow seeds of discord about matters on which there should be no orthodoxy. I commend Grendler for an impressive contribution to Renaissance studies.

DONALD NUGENT
*University of Kentucky*

MARVIN R. O'CONNELL. *The Counter Reformation, 1559–1610*. (The Rise of Modern Europe.) New York: Harper and Row. 1974. Pp. xv, 390. $10.00.

The title notwithstanding, this latest addition to the famous "Langer Series" is intended as a survey of all aspects of European history during the half-century between the two dates assigned to it by the general editor. Hence, it properly should be evaluated in the context of the many other general surveys upon which it admittedly depends for much of its specific information.

Needless to say, it more accurately represents the current state of historical scholarship than the venerable values in the Lavisse and the Fliche-Martin series. It is internally more coherent than the rather uneven anthological volume of almost the same title in the "New Cambridge Modern History." It is more limited in time, and hence more thorough within those limits, than the corresponding volume in Denys Hay's "General History of Europe." It is more thematically organized than the one in Felix Gilbert's "Norton History of Modern Europe." It is broader in geographical scope—and considerably less expensive—than the ones in the "Le Fil des Temps" and the "Nouvelle Clio" series, and it is broader in topical scope than those in the "Storia Politica d'Italia." To be sure, it lacks some of the features of the few surveys it does not cite: the full bibliographical apparatus of the most recent manuals published in Germany, the ideological commitment of the most recent *liceo* texts published in Italy (e.g., Giorgio Spini and Giuliano Procacci), and the careful examination of sources characteristic of new urban histories (Toulouse, Naples, Mantua, Valladolid, and, above all, Milan). But in combining comprehensiveness with clarity it approaches the high quality of the two texts the author rightly indicates, by the abundance of his references to them, as the best of the

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genre: J. H. Elliott's *Europe Divided* (in J. H. Plumb's "History of Europe") and Owen Chadwick's *The Reformation* ("Pelican History of the Church").

What distinguishes this survey from most of its predecessors, however, is its success in avoiding their principal pitfall: the reduction of history to a dull list of *événements*. The author never lets the standard of impersonal objectivity dampen his enthusiasm for subjects he himself has studied in depth—like the career of Thomas Stapleton. He never lets previously proposed schemes of organization dictate his own; and he accordingly allots six pages to Scotland and none at all to Bavaria, Tuscany, and Naples, because he thinks the former correspondingly more important than the latter. He does not permit the claims of more recent writers to infringe upon the lasting authority of his old favorites—Jean Mariéjol, R. B. Merriman (his chief source for Spain), J. W. Thompson, Antonio Battistella, and even Henry Daniel-Rops; and luckily the old favorites betray him at only one point: when they force him, *pace* Gaetano Cozzi, to isolate Venice from the rest of Europe as the only "secular, . . . anti-ecclesiastical" state of the age, "convulsed by internal difficulties" (p. 268). He brings the information he presents to bear upon general theses regarding the age as a whole, and he defends these theses against their opposites—like those of Hugh Trevor-Roper on the Counter Reformation and those of the critics of Philip Hughes on the English Reformation. He adds *bon mots* of his own to those he quotes liberally from his rhetorically conscious heroes. And he artfully varies the standard metaphors of scholarly prose ("a successor cut out of different cloth," p. 83) with his own more striking metaphors ("the quicksilver was running the other way," p. 247).

Moreover, except for a brief bow to a Braudellian format in the first chapter (geography, demography, economics), and except for the addition of a bit of *Kulturgeschichte* in the last chapter, the author remains courageously faithful to the kind of history writing that was one of the great achievements of the age he writes about. He does so, indeed, in spite of a marked distaste for Renaissance humanism in general (e.g., pp. 86, 97, 119). History is made, he insists, or at least history was made in the sixteenth century, not by inexorable changes in technology, economic productivity, or social organization, but by a small number of individual persons (e.g., "the peace . . . depended upon the strength of one slender woman," p. 156); and history therefore consists almost exclusively in the political and military affairs with which those persons were chiefly concerned. The book abounds in poignant character sketches, some of them worthy of a Guicciardini—Gaspard de Coligny with a toothpick ever dangling from his mouth, Alexander Farnese, the "charming, jovial, charismatic" leader of the Netherlands loyalists, Henry III, "intelligent, charming . . . , [but] devious, perverse, and worst of all, lazy" (p. 248). Individual persons are the chief, if not the sole, agents in history; and the chain of historical causality must therefore begin not with price curves, *structures*, or *conjonctures*, but with the peculiar complexion of their personalities. The book also abounds in lively narrative: the murder of Mary Stuart's Savoyard favorite (p. 158), the flight of William of Orange from Mons (p. 172), and, the longest and most dramatic of them all, the Battle of Lepanto. It is such events as these that mark the chief turning points in history. Accordingly, the most that the author will grant to the modern rage for graphs and charts is a map showing the various positions of the Christian and Turkish ships at the mouth of the Gulf of Corinth in October 1571.

Such fidelity to humanist historiographical standards will certainly please all those who, like myself, still find Machiavelli and Paruta profitable as well as enjoyable to read. Yet when these standards are applied rigorously to what the author rightly perceives as the most important aspect of European history during this period, they seem to impose upon him a view that is very close to the one put forth long ago by the father of Counter Reformation studies, Ludwig. von Pastor. The Counter Reformation, says O'Connell, was largely the work of a few energetic popes assisted by a few dedicated bishops and a few learned theologians. Its geographic limits were determined largely by the outcome of battles and diplomatic negotiations. Its principal achievements consisted in a thorough restructuring of the Roman bureaucracy and in "the emergence of a papal monarchy" (p. 112). And it can best be described as a "conservative . . . restoration."

In spite of its age, this view is by no means obsolete. It has been supported in the last several decades by Pietro Tacchi Venturi and Mario Scaduto, who drew a generic distinction between corruption before and vitality after the pontificate of Paul IV. It has been supported more recently by Philip Hughes, whose negative judgment of the German Counter Reformation O'Connell nonetheless rejects, and by Philip McNair, whose negative judgment of the religious elements of Italian humanism he apparently accepts. And it was supported most eloquently just two years ago by Romeo de Maio (*Riforme e miti nella Chiesa del Cinquecento*), who discounted the whole Catholic Reform movement as a projection into the past of Hubert Jedin's misreading of the works of a few isolated intellectuals.

Yet this view is not wholly consistent with those of a number of other recent scholars. For instance, Ernesto Cattaneo, Richard Douglas, Adriano

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Prosperi, Giuseppe Alberigo, *et al.*, have added considerably more evidence in support of Jedin's thesis about the durable effects of "reform from below." Paolo Prodi and the several students of the *Einführung des Tridentinums* in Germany have found that the post-Tridentine bishops had to rely mostly on their own initiative and that they therefore differed considerably in the means they adopted for the implementation of the conciliar decrees. Prodi and Mario Bendiscioli have shown that the Counter Reformation's most famous "model bishop," Carlo Borromeo, owed much of his success in overcoming opposition both from the Spanish governor and from the Roman Curia to the enthusiastic support of his ecclesiastical subjects; and Federico Chabod has shown this support to have been the result of some forty years of autoreformation among the subjects before the arrival of the bishop. Ernst Walter Zeeden, Massimo Petrocchi, and Alberto Vecchi have drawn attention to a completely different aspect of the Counter Reformation, one which O'Connell leaves out: the often radical changes that took place in the realm of religious piety. The many scholars who trace the activities of the sixteenth-century missionaries have generally accepted the thesis of the missionaries themselves about the world-wide character of the Counter Reformation—a thesis O'Connell does not accept in limiting his view of it solely to continental Europe. The scholars who have been inspired by the methods of Gabriel le Bras are now busy testing what is today the most provocative working hypothesis in the field, that of Jean Delumeau—a hypothesis that happens to be exactly the opposite of O'Connell's. The Tridentine reforms did not push Catholic Europe back toward the Middle Ages or the Age of the Fathers, says Delumeau. Rather, they transformed the still largely pagan society described in fifteenth-century Flanders by Jacques Toussaert into the thoroughly Christianized society described in the seventeenth-century Ile-de-France by Jeanne Ferté.

Given the chaotic state of Counter Reformation studies today, it is impossible to say which of these many views is correct. But this book has made a vital contribution toward clearing up the confusion. It has done so not by proposing a generally acceptable definition of a long misunderstood phenomenon, but by opening a long overdue debate—a debate from which, we hope, such a definition can eventually emerge.

ERIC COCHRANE
*University of Chicago*

CYRIAC K. PULLAPILLY. *Caesar Baronius: Counter-Reformation Historian*. Notre Dame: University of Notre Dame Press. 1975. Pp. xiv, 222. $12.95.

Baronius rose from obscure noble origins in the Neapolitan hinterland to become the father of modern church history. For some years he was probably the most influential cardinal in the Roman curia, and in 1605 he was twice nearly elected pope, despite his obvious unwillingness to hold the office. Cyraic K. Pullapilly asserts convincingly that Baronius' life and his life work, the *Annales Ecclesiastici*, must be considered together, and he follows chronologically their parallel development.

Throughout his life the basic goal of this pious and upright cleric was "the preservation and strengthening of the institutional Church" (p. 134). This was as true of his writing as of his curial activity, for example, his successful advocacy of the absolution of Henry IV and his support of the Venetian Interdict. Thus he was "a Counter-Reformation prelate in the strongest sense of the word" (*ibid.*).

In the last chapter, devoted to an appraisal of Baronius as a historian, Pullapilly describes his herculean efforts to write a comprehensive history of the Church based on all available sources, and notes the enormous stimulus his work gave to ecclesiastical history. Pullapilly attributes Baronius' insufficient critical spirit in part to his apologetic purpose, especially in his intent to refute the Protestant *Magdeburg Centuries*.

Making particular use of the Cardinal's papers in the Vallicelliana Library in Rome, Pullapilly has given us a good introduction to Baronius which clearly shows his importance. The book is well-written except for occasional clumsiness of expression and a lack of clarity in the final chapter. Moreover, the discussion in this chapter of Baronius' relationship to different historiographical traditions is unsatisfying. The treatment of his conception of history and his historical method suffers from an inadequate understanding of theology. Pullapilly's characterization of Catholicism as "a religious ethos in which authority or tradition enjoyed the same status as revealed truth" (p. 163) is uninformed.

Despite shortcomings, this is a most welcome brief life which may stimulate someone—perhaps Pullapilly himself—to undertake the definitive biography Baronius deserves and may encourage more American historians to turn their attention to papal history.

ROBERT BIRELEY
*Loyola University, Chicago*

JAMES TURNER JOHNSON. *Ideology, Reason, and the Limitation of War: Religious and Secular Concepts, 1200–1740*. Princeton: Princeton University Press. 1975. Pp. x, 291. $12.50.

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James Turner Johnson's work seeks to trace the "interaction between religion and secular society" in the history of just war doctrine from the Middle Ages to the eighteenth century, and to interpret this doctrine as the expression of a broad human urge to limit organized violence which deserves new codification today. No brief study can encompass the political, military, and theological developments that made up the rise and fall of the doctrine. For instance, just war in the Middle Ages, a subject on which Frederick H. Russell recently published an important book, is here treated in one chapter. By the "interaction between religion and secular society" the author really means the relationship between theological and secular doctrines of the just war, exemplified by arguments of selected writers. The opening chapter shows how these two bodies of thought combined to create the classic doctrine. The second chapter discusses the development of the doctrine of holy war in England. In contrast to Bainton and Walzer, the author finds that not only Puritans but many groups in English society thought of war in terms of a crusade, which, however, they were prepared to wage with a measure of Christian charity. The third and fourth chapters trace the secularization of just war thought into a nonideological doctrine emphasizing *jus in bello* rather than *jus ad bellum*. An epilogue speculates on possibilities of establishing new ethical limitations on the use of war.

I have two reservations about Johnson's thoughtful analysis. He is highly selective, but his principle of selection is never stated. To prove that Puritans were not alone in thinking of war as a crusade it suffices to point to the writings and actions of this or that seventeenth-century English Catholic. But a study of the many facets of just war doctrine over the centuries must either be more encompassing or explain why it is not. Johnson discusses Raymond of Penafort but not Innocent IV (except for one reference), Bacon but not Thomas More, Suárez but not Bellis or Ayala. The result is not simply a lack of nuance; such major issues as preventive war receive unduly impressionistic treatment. The second question concerns the lack of systematic analysis of the pragmatic counterpart of the just war, the doctrine of *raison d'état*. From the thirteenth century on, theories of the just war were usually formulated in response to the concept of sovereignty and later to claims of *raison d'état*. The author's decision to say little about these new forces means that just war doctrine emerges from his discussion as even less relevant to political thought and behavior than it actually was. Even as a condensed study of the just war in European history Johnson's book is too fragmentary, but it is worthy of study for its close analysis of aspects of this doctrine and for its exploration of some traditional remnants in contemporary American thought on the morality of war.

PETER PARET
*Stanford University*

PETER PARET. *Clausewitz and the State*. New York: Oxford University Press. 1976. Pp. viii, 467. $18.95.

Karl von Clausewitz has been more often the source of striking quotations than the subject of systematic investigation, partially because the difficulties in understanding him are formidable. Most of his papers vanished at the end of World War II, and the dating of the remainder is sometimes problematic. No printed collection of his writings is complete, and much is available only in fragments. His greatest work, *On War,* was not finished at his death, and newer ideas—notably, as Peter Paret convincingly argues, the acceptance of the reality of two types of warfare: absolute and limited—were not integrated into all of the text.

With unusual meticulousness, clarity, and precision, Paret has concentrated his examination on the development of Clausewitz's character and thought in the context of his time. The result is a rare combination of critical biography with penetrating theoretical analysis. The political, social, intellectual, and military currents which influenced his life and ideas are skillfully blended. There is an especially sensitive evaluation of the respective weight philosophy and events played in producing Clausewitz's singular amalgam of idealism and realism.

Particularly impressive is Paret's scrupulous methodology. Psychological analysis is applied, for example, to the springs of Clausewitz's strong ambition, but, wisely in view of the sparseness of sources, Paret lets the reader make the final judgment. He reappraises the last years and, in polite conflict with Hans Rothfels and Werner Hahlweg, discounts Clausewitz's pessimism, concluding, after a look at the careers of others, that there were no objective reasons for professional disappointment. Comparative method is, as in Clausewitz, one of the outstanding merits of this book. The Prussian military establishment is silhouetted against the French and Clausewitz against his contemporaries in a manner which increases our understanding.

This carefully laid mosaic of biography, history, and theory, based upon subtle use of sources, sparkles with insights. The author persuasively denies Clausewitz's overemphasis on the importance of battle; painstakingly delineates the influences of Machiavelli, Fichte, and Pestalozzi; and underscores Clausewitz's objectivity and his emphasis on historical experience, emotion and chance, and

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"friction" in evaluating warfare. While Clausewitz is viewed sympathetically, there is no exaggeration of his role; his warts, including fervent dislike of Jews and Poles, are clearly evident.

The unifying theme is Paret's contention that Clausewitz held that the state, not the army, must be the final arbiter of military action. In this, as in much else, he was remarkably consistent, spending his life as a realistic, undoctrinaire, and equally unrevolutionary critic of the Prussian state and society.

This distinguished work, now the best introduction to Clausewitz, encourages the hope that Paret will cap it with a detailed analysis of the contents and influence of *On War,* which Michael Howard and he have newly translated.

FREDERIC B. M. HOLLYDAY
*Duke University*

HARRY C. PAYNE. *The Philosophes and the People.* (Yale Historical Publications, Miscellany, number 109.) New Haven, Conn.: Yale University Press. 1976. Pp. x, 214. $12.50.

Harry Payne's thesis is that the characteristic attitude of the philosophes toward the people, that is, the downtrodden majority of eighteenth-century European society, was highly ambivalent. Toward them the philosophes showed contempt, scorn, and condescension curiously mixed with genuine sympathy and concern for their plight, which increased as the century progressed and the Enlightenment matured. This thesis is not new, nor is Payne's version of it particularly original. Almost every major study of the Enlightenment points out its inconsistencies and ambiguities. But most, including Payne's, throw little light on why these ambiguities appeared, why the philosophes failed to resolve or transcend them, and how they reflected the tension between the ideals of the philosophes and the historical reality to which they belonged.

Instead, Payne compiles the characteristic Enlightenment theories of politics, economics, religion, and education and examines how the philosophes fashioned them into tools for the improvement of the people's lot. These chapters are, on the whole, clear, well organized, and intelligent. They reveal, thoughtfully and convincingly, significant relationships between the theory and practice of the philosophes. Some of Payne's conclusions are open to question, especially in regard to the German Enlightenment about which he seems to know much less than he does about the French and English. He seems to take for granted, for example, that the German *Sturm und Drang* represented a repudiation rather than an adaptation of the French Enlightenment (p. 31). Several important works, which might have strengthened Payne's conceptual framework, are absent from his bibliography: Crocker's many studies, Horkheimer's and Adorno's *Dialectic of Enlightenment,* Brunschwig's *Enlightenment and Romanticism in Eighteenth-Century Prussia,* and Lukác's *Goethe and His Age.* In sum, Payne's book has its good points, but it is not the significant contribution to Enlightenment scholarship that it purports to be.

ROBERT ANCHOR
*University of Southern California*

PAUL MONACO. *Cinema and Society: France and Germany during the Twenties.* New York: Elsevier. 1976. Pp. viii, 194. $9.95.

The author of this provocative work on the relationship of film to society has studied the most popular feature films produced in Germany and France during the 1920s and discovered that each national cinema contained certain themes and symbols that were virtually unique to it. For example, in French films such motifs and symbols as the individual abandoned in the world, the orphan, and blood tend to dominate. By contrast, the German cinema of the period emphasized themes of betrayal, dangerous and ugly streets, suicide, and unseen blood.

One might simply attribute this phenomenon to coincidental or esthetic considerations, but Paul Monaco believes that the specific themes to be found in a nation's cinema are a reflection of the shared, collective concerns of the masses to which these films appeal. His thesis, based in part on Freudian dream theory, is that there exists a unique kinship between dreams and films: just as dreams have a latent meaning for the individual, films are the unconscious reflection of a nation's psyche, a medium through which the innermost wishes and fears can be expressed. Monaco reasons that most films that become box-office successes owe their appeal to the fact that they touch on the inner concerns of the group.

Even if one accepts the author's thesis as fundamentally valid, one might feel that there is a certain arbitrariness and incongruity in many of the interpretations he offers. To give just one example, the argument that the particular kind of love triangle that was central to many French films was a reflection of France's relations with her Eastern European allies (a substitute partner) and England (the true lover from whom France had become estranged but with whom she yearned for a reconciliation) is unpersuasive, not the least because of the unlikelihood that ordinary Frenchmen

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had such thoughts either in everyday life or in their unconscious.

Yet some of the author's suggestions are quite intriguing and insightful. The prevalence of children as important figures in French films, but not in their German counterparts, could very well be, as Monaco suggests, an unconscious expression of concern over the grim demographic trends of which most Frenchmen were aware. Thus, even though many readers will reject some of the author's theses and conclusions, the student of film and history will probably find the book to be as stimulating and, in certain ways, more sophisticated than its influential but much maligned predecessor, Siegfried Kracauer's *From Caligari to Hitler*.

THOMAS L. SAKMYSTER
*University of Cincinnati*

D. M. LOADES. *Politics and the Nation, 1450–1660: Obedience, Resistance, and Public Order*. Brighton, England: Harvester Press; distributed by Humanities Press, Atlantic Highlands, N. J. 1975. Pp. 484. $13.50.

This book is a well-written and carefully researched story of the rise and fall of Tudor monarchy. The author chose to begin his study with the 1450 rebellion of Jack Cade because it symbolized the Lancastrian failure to maintain "good lordship." This medieval concept of cooperation between the crown and the "political nation" was explained in part as the duty of the monarch to seek advice from and to reward his magnates (aided by a system of clientage) for their assistance in war and law enforcement. Historians have chosen other beginning dates (1399 and 1471, for example) for studies of this period, but the unifying theme of "good lordship" makes this author's choice a coherent and meaningful one.

"Good lordship" was revived first by the Yorkists and then by the Tudors, who were able to destroy the magnates' independence by utilizing them and the knightly class in an ever-expanding and more comprehensive royal machinery, thus fostering a dependence of both groups on the central authority. Royal power was further enhanced by the absorption of secular franchises and ecclesiastical jurisdiction. Nevertheless, Tudor success continued to depend upon cooperation with the nobility, and, especially, the gentry, who increased in numbers and wealth during this period. Freed from military and social dependence upon their noble superiors, the gentry, dominating the House of Commons, began to discover "the luxury of political opposition" to the crown.

With the breakdown of "good lordship" in the Stuart era, this "country" opposition became alienated from the court and resorted to drastic constitutional measures. The civil wars that broke the ensuing political deadlock led to revolutionary settlements initiated by radicals outside the traditional "political nation." After 1660, however, the propertied classes became the "masters" of the surviving administrative machinery of the Tudor monarchy, but its centralizing prerogative and fiscal authority was not restored.

It is refreshing to read this perceptive political narrative which adroitly used the theme of "good lordship" to draw together the 110 years under consideration. This book is an excellent counterbalance to the many social and economic studies of the English civil wars.

RETHA M. WARNICKE
*Arizona State University*

W. H. BRYSON. *The Equity Side of the Exchequer: Its Jurisdiction, Administration, Procedures and Records*. (Cambridge Studies in English Legal History.) New York: Cambridge University Press. 1975. Pp. xiii, 217. $22.50.

When we think of the Exchequer, the king's financial department comes to mind. But the Exchequer had another side, an equity one, about which W. H. Bryson has written a thorough and scholarly monograph. His work complements those of W. J. Jones, D. E. C. Yale, and G. Spence on Chancery, maintaining the high standards that they have set.

Exchequer's equity jurisdiction grew out of its financial responsibilities. Originally, it heard cases that touched on the king's financial interests. By the sixteenth century three sorts of plaintiffs were allowed to sue in Exchequer: its own officers, any civil servant obliged to account for revenue due the crown, and any subject who owed the king money. After the Civil War Exchequer grew from a specialized revenue court into a general equity one, filling a void left by the abolition of such equity courts as Star Chamber and those in the North or Welsh Marches. By broadly defining its jurisdiction the court was able to admit a wider range of plaintiffs and suits, hearing, for instance, disputes over tithes, until by William III's reign it was handling some seven hundred cases a year. For over a century, Exchequer continued to be a major equity court, second only to Chancery. An act of 1817 inadvertently brought about the court's demise; by allowing the chief baron to hear cases on his own, it curtailed appeals, forcing lawyers to remove their business to Chancery. Thus in 1841 Parliament suppressed the equity side of Exchequer, making it once more an exclusively financial department.

Apart from chronicling the court's rise and fall, Bryson describes the duties of all its officers, providing lists of incumbents, with short biographical

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sketches. For the greater officers, the treasurers and chancellors, men such as William Cecil, Lionel Cranfield, or Sidney Godolphin, such sketches are superfluous. For the lesser officers, such as the four barons, the sworn clerk, the king's remembrancer and his deputy, Bryson could have made more of his considerable diligence; for instance, a prosopographical analysis might have told us much about the careers of minor legal officials.

A similar criticism may be made of Bryson's extensive description of the court's records; his purpose, he tells us, is to open them up to other historians. While he says much about where they may be found, the forms that were used, and how the records were created, he says little about what sort of information is in them. This omission is a pity, for many scholars, in what one might call the new legal history, have used court records to investigate subjects as diverse as widowhood or marriage in Elizabethan England, or old age in Colonial America.

Bryson has set out to write a traditional legal history, and in that limited goal he has succeeded admirably. If his monograph attracts other historians to study the equity side of the Exchequer as something more than a mere court of law, then his diligence will have been even more fruitful.

CHARLES CARLTON
*North Carolina State University*

CHARLES H. LYONS. *To Wash an Aethiop White: British Ideas about Black African Educability, 1530–1960.* (Studies in International Education.) New York: Teachers College Press. 1975. Pp. xv, 192. $7.50.

*To Wash an Aethiop White* undertakes to delineate concepts of racial equality and inequality primarily as they reflect Britons' ideas about Africans' educability. Early stereotypical thinking about blacks' inferiority was grounded in perceptions of color difference and cultural strangeness which bred unconscious prejudice. Later, racial beliefs reflected changing scientific theories: in the eighteenth and nineteenth centuries, theories concerning physiognomy, climatology, phrenology, or craniology; in the nineteenth and twentieth centuries, evolution, structural-functional anthropology, and genetics. They were also shaped by sociocultural phenomena like the late eighteenth century's missionary and humanitarian reform movements, the early nineteenth's antislavery crusades in England and abroad, and even the esthetics enshrined in changing artistic canons and popular preferences. More consciously, they were manifested in public policies which, in the eighteenth century, encouraged education of Africans to benefit trade; then discouraged it because it created a glut of educated Negroes; and finally returned to educating Africans under Indirect Rule to train a native administrative cadre.

Charles H. Lyons surveys most of this ground and more. Nonetheless, his is a disappointing book. He cites a reasonable amount of the primary data, but leaves his reader feeling he has encountered the substance before—better and more fully developed. So it turns out. One-third of the footnotes in the book's first 110 pages cite three books: Philip Curtin's *Image of Africa,* Thomas Gossett's *Race,* and Winthrop Jordan's *White Over Black.*

It may be objected that these books and the somewhat less frequently cited *Leopard's Spots* by William Stanton or Christine Bolt's *Victorian Attitudes to Race* deal primarily with general race ideas, not "educability." That they do so pinpoints Lyons' conceptual problem. In reality his "educability" embraces not only concepts of natural endowment (to which most of the scientific thought speaks), but also cultural perceptions of what *kind* of education is desirable for humane, religious, or other reasons, and policy imperatives, which define the sorts of education designed for predetermined politico-economic ends, as well. Because he fails systematically to define or analyze these variations, Lyons cannot deal with them effectively. He therefore settles for a limited survey of race thought, bounded more or less by "British," "African," and "education." One hopes that he will, in the future, tackle the conceptual analysis. Meanwhile, we shall continue to read and reread our Curtin, Jordan, and the rest.

WILLIAM H. PEASE
*University of Maine, Orono*

NANCY LENZ HARVEY. *The Rose and the Thorn: The Lives of Mary and Margaret Tudor.* New York: Macmillan Publishing Company. 1975. Pp. xiv, 270. $10.95.

Nancy Lenz Harvey has written an interesting account of the lives of Margaret and Mary Tudor, both of whom figured significantly in England's diplomacy with Scotland and France. During a period of much Anglo-Scottish conflict, the pro-English Queen Margaret intrigued at times with her brother, Henry VIII, who wished to annex Scotland. At other times, when Henry neglected her monetary requests, which were many, she aligned with the Duke of Albany, the French regent, thereby infuriating Henry VIII. Her three marriages to James IV, the Earl of Angus, and Lord Methven were all stormy and filled with much unhappiness. The union of England and Scotland was, however, eventually accomplished

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through the offspring from her marriage to James IV.

Mary Tudor, who was pawned off in a diplomatic marriage to the aging Louis XII, served as French queen during the few remaining months of Louis XII's life. Upon his death, she quickly arranged a secret marriage to Charles Brandon, Duke of Suffolk, with whom she had been in love for some time. Mary's second marriage was a relatively happy one, in sharp contrast to the almost constant unhappiness of her sister Margaret.

The author has gleaned rather well from the printed sources, with some notable exceptions such as Marie Stuart's biography of the Duke of Albany, Teulet's edited works on Franco-Scottish diplomatic correspondence, and J. H. Burton's eight-volume *History of Scotland*. A few manuscript sources are cited from the British Museum, but no evidence is given of work in the National Library of Scotland or the *Bibliothèque Nationale* in Paris. Although the narrative is handled effectively, there is little new information presented in this work. For a study of Mary Tudor, W. C. Richardson's *Mary Tudor, The White Queen* is probably more thorough, and Hester W. Chapman's *The Sisters of Henry VIII* contains most of the essential information found in Harvey's book.

Despite the aforementioned limitations, Harvey's work is a welcome addition to the literature of the early Tudor period. The book is well written, footnoted, and indexed, and it includes several excellent portraits of leading figures involved in the study.

RICHARD GLEN EAVES
*Auburn University*

DAVID KNOWLES. *Bare Ruined Choirs: The Dissolution of the English Monasteries*. New York: Cambridge University Press. 1976. Pp. 329.

This is a splendidly illustrated abridgment of David Knowles's *The Religious Orders in England*, vol. 3: *The Tudor Age*, first published in 1959 (rev. *AHR*, vol. 66, Oct. 1960). The ten appendixes, the bibliography, and all the scholarly apparatus have been removed from the original work. It is therefore intended for a wider audience than the academic and was completed by Knowles just two weeks before his death. While one would not quarrel with the few textual deletions, most of which deal with the period before 1530, it is regretable that Knowles chose to omit his brilliant critique of Erasmus and his influence on the sixteenth-century world view. Since 1959 two studies have been published on the suppression of the English monastic and religious houses, both titled the *Dissolution of the Monasteries,* and both of which attempt a broad treatment of the subject: that of G.W.O. Woodward (1966) which actually focuses on the Yorkshire houses, and that of Joyce Youings (1971), whose collection of documents is useful for undergraduate teaching. Knowles's *Religious Orders*, volume 3, remains the definitive scholarly treatment.

The history of the dissolution of the monasteries has been described many times, but never with such depth of learning or such profound understanding. The suppression was a result of the crass materialism of the age, a blind submission to the king, and the decline within the monasteries of religious fervor and of fidelity to the *Rule*. The dissolution did not bring about a better distribution and use of land and wealth or advance the cause of social justice or serve any national purpose. Rather, the "Bare Ruin'd Choirs"—the title is, of course, from Shakespeare's 73rd Sonnet—testify to the loss of an imponderably valuable cultural and esthetic force and a significant spiritual influence.

Knowles once said this book contained more of himself than any other. A poignant sadness pervades the book, but it is never sentimental. It also reflects his absolutely magisterial knowledge of the history of the period, a rare appreciation of English literature, and a prose style of exceptional beauty.

Father David Knowles died on the 21st of November 1974 the date observed, as it happens, as the *Dies Memorabilis* in the English Benedictine Congregation because on that day in 1607 the Congregation was re-established.

BENNETT D. HILL
*University of Illinois, Urbana-Champaign*

M. L. BUSH. *The Government Policy of Protector Somerset*. Montreal: McGill-Queen's University Press. 1975. Pp. x, 182. $18.25.

M. L. Bush's study of Protector Somerset's government policies is an important, provocative, and original interpretation of a man whose motives have been consistently idealized and, according to Bush, hopelessly misunderstood by those who view the "good Duke" as the harbinger of liberty, equality, and social welfare. The central thesis of the book is that Scotland and the Scottish war were the Protector's paramount concerns and the key to all his governmental actions. His religious, social, and economic policies were all modified, curtailed, or dictated by the exigencies of war. Had it not been for the Scottish conflict the Duke's government might have recognized the most obvious cause of inflation—the debasement of the currency—and have refrained from chasing after those relatively innocent scapegoats for economic

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woe: the sheep farmer and encloser. Had it not been for the fear of antagonizing the Emperor Charles and isolating England during wartime, Somerset would have refused to consider a domestic religious compromise well to the right of his own inclination.

Practicality, necessity, circumstance, stopgap measures, concern for appearances, and typical Tudor paternalistic desire for law and order, not farsighted social, religious, and economic aspirations or a deep concern for the downtrodden, were the operating factors behind the Duke's policies. As a man, Somerset emerges as neither a hero nor a villain, but simply a military leader of limited talents who was essentially in tune with the prevailing social and economic climate of his day. Bush's interpretation has most of the flaws of a single cause explanation, and his book reads at times like a series of lectures—repetitive and militant. Nevertheless, Somerset himself emerges as a much more believable, indeed more attractive and sympathetic, character; and Bush's thesis, though overstated, is by and large convincing.

LACEY BALDWIN SMITH
*Northwestern University*

STEPHEN GARDINER. *A Machiavellian Treatise*. Edited and translated by PETER SAMUEL DONALDSON. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1976. Pp. ix, 173. $22.50.

By publishing a surviving Italian translation of the last treatise of Stephen Gardiner, bishop of Winchester, Peter S. Donaldson has presented important new evidence concerning both Marian politics and Machiavelli's influence in England. Donaldson also provides an English translation and an erudite introduction setting the treatise in its political context, 1553–1555. Gardiner, whose opposition to Mary I's Spanish match can no longer be taken at face value, apparently intended his treatise to serve Philip II as a guidebook for perpetuating Habsburg rule in England. English dynastic history is selectively presented, and the political counsel to be derived from it is borrowed without acknowledgment from Machiavelli. Like Machiavelli, Gardiner subordinates religion and morality to political considerations: his purpose is to show how a prince can keep his state, which often necessitates acting "contrary to mercy, religion, and faith" (p. 149). Gardiner, who practically ignores Mary, clearly indicates the popular policies that Philip should pursue to maintain "his" state. He does not deal directly with the question of a Habsburg succession in the eventuality of Mary's death without issue—which, according to English statute law, would mean the accession of Elizabeth. He does, however, suggest the Machiavellian answer in telling how several English kings expediently exterminated their potential rivals. The evidence of Gardiner's treatise controverts the still common view that there was little knowledge of Machiavelli in Tudor England. It also indicates that contemporaries came closer to the truth about Bishop Gardiner than his modern apologists when they called him "wily Winchester."

MORTIMER LEVINE
*West Virginia University*

ROSEMARY O'DAY and FELICITY HEAL, editors. *Continuity and Change: Personnel and Administration of the Church in England, 1500–1642*. Leicester: Leicester University Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1976. Pp. 303. $20.25.

Some of the best work on the English Reformation is being done by young English scholars who are increasingly utilizing local sources to discover what the state of the Church within a given region actually was. This volume brings together essays by nine such scholars. The studies are unified by their concern for constitutional and administrative history rather than theology or politics and by their attempt to show that many pre-Reformation institutions survived, but were altered fundamentally from within during the century preceding the Civil War.

Several studies limit themselves to a single diocese. Two of the new bishoprics founded by Henry VIII are examined: Chester, by Christopher Haigh, and Peterborough, by William Sheils. Stephen Lander writes on the ecclesiastical courts in the diocese of Chichester, advancing the convincing thesis that these organs were undergoing reform in the last decades before the break with Rome but were permanently weakened by attacks upon them during the 1530s. A piece by David Marcombe considers the extent to which the dean and chapter of Durham were but the old abbey writ large.

More general studies of institutions include a very useful analysis of procedures and problems involved in collecting taxes from the clergy—this by one of the editors, Felicity Heal—as well as a new look at the Prerogative Court of Canterbury by Christopher Kitching and an account of the role of the registrar in diocesan administration by Rosemary O'Day. The book begins with Andrew Foster's biographical essay about that influential Arminian, Richard Neile, and concludes with Ralph Houlbrooke's evaluation of the decline of ecclesiastical jurisdiction under the Tudors.

All of these chapters are carefully researched and intelligently written. The topics may seem narrow, but they are important. The cumulative

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effect of studies such as these will be a fresh understanding of the Reformation, not merely as it was ordained by political leaders, but as it actually affected the lives of ordinary English clergy and laymen.

STANFORD LEHMBERG
*University of Minnesota, Twin Cities*

HORTON DAVIES. *Worship and Theology in England.* Volume 2, *From Andrewes to Baxter and Fox, 1603–1690.* Princeton: Princeton University Press. 1975. Pp. xxiii, 592. $25.00.

Although readers should recognize the weaknesses and limitations of Horton Davies' last book on English worship, he has composed a masterful finale to his five-volume set. Over the past fifteen years Davies has surveyed English worship from the post-Reformation times of Archbishop Cranmer through the ecumenical aspirations of the twentieth century. His final volume retains the ecumenical design of the entire set by looking at elaborate forms of Anglican and Catholic worship as well as the simple but inspiring expressions of Puritans, Baptists, and Quakers.

Part one is a clear presentation on church architecture, spirituality, and preaching. Davies provides insights on differences in worship, such as between the Anglican view of a devout nation or the Puritan sense of a covenanted fellowship of households. The mentality of the different worshipers appears in part two on cultic controversies. Puritans preferred a scriptural, simple style of worship, Quakers favored the spontaneous, and the Catholic-Anglican wing expressed itself with more order and respect for tradition. Part three provides an unparalleled survey of the forms of worship with special attention to the service books. Throughout he concentrates on major figures, such as Lancelot Andrewes, Richard Baxter, and George Fox, but he frames careful profiles of John Donne, Christopher Wren, James Ussher, Gilbert Sheldon, John Owen, William Laud, Thomas Goodwin, and John Evelyn. Davies concludes with a critique of the various forms of worship. He maintains that liturgical worship has advantages, such as order and visibility, but recognizes it can become spiritless. Puritan worship too had strengths in its reliance on Biblical authority and in its simplicity, but it was marred by its extreme didacticism and foolish rambling. The Quaker and Baptist modes brought flexiblity and interiority, but this, although Davies does not mention it, sometimes led to inaction or unrestrained, divisive enthusiasm. In the end Davies hopes that his reader will recognize that the various forms of worship will complement and unite rather than divide and contradict.

Although the series purports to survey worship and theology, as in the other volumes Davies does not explore theology in depth. Yet Arminianism, Caroline moral theology and the Puritan theologies of William Ames or Richard Baxter had great influence. In some chapters (2, 4, 7) where he relied on existing authorities, he could have included more on theology. The section on the context of worship describes the unpretentiousness of the Dissenters and the achievements of the Anglicans, but it frequently turns into an architectural survey and not a direct treatment of worship. A shorter discussion, combined with more pictures of churches and other objects used in worship, would have served the purpose and saved more space for theology. Also, the organization does not permit a sense of the ongoing disputes and crises in the century. The problems of the Puritans early in the century, the effect of persecution on worship, the emergence of the Baptists and Quakers, and the changes both in 1662 and after 1690 lose themselves in an analytical rather than a chronological structure. Finally, Davies has not surveyed, or at least acknowledged, recent dissertations, monographs, or manuscripts.

As with past volumes this work has limitations of perspective, structure, writing, and scholarly definitiveness, yet it is a masterful survey. The clarity of organization, the concise statements of positions, the balanced assessments, the occasional brilliance of phrasing, and the wealth of insights make it worth the price.

KENNETH W. SHIPPS
*Trinity College*

ALLEN G. DEBUS, editor. *Medicine in Seventeenth Century England: A Symposium Held at UCLA in Honor of C. D. O'Malley.* (Sponsored by the Department of Medical History at the University of California, Los Angeles, and supported by the Josiah Macy, Jr. Foundation.) Berkeley and Los Angeles: University of California Press. 1974. Pp. xiii, 485. $15.00.

C. D. O'Malley, a Renaissance scholar and chairman of the Department of the History of Medicine at UCLA, sponsored a series of international symposiums on medical history. This present volume is a collection of papers from the final symposium which O'Malley organized but did not live to see. The participants include most of the better-known medical historians working on the seventeenth century, and the result is an extremely useful volume.

Sixteen of the seventeen essays deal with various aspects of seventeenth-century medicine, and they

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range from the philosophical, such as Lester King's "The Transformation of Galenism" and Charles Bodemer's "Materialistic and Neoplatonic Influences in Embryology," to the more pragmatic, as exemplified by Samuel Radbill's "Pediatrics" and James Cassedy's "Medicine and the Rise of Statistics." While the names of Thomas Willis, Thomas Sydenham, and William Harvey constantly recur, in the view of modern medical historians Willis has clearly replaced Sydenham as the English Hippocrates, and L. J. Rather in his "Pathology at Mid-Century: A Reassessment of Thomas Willis and Thomas Sydenham" does a masterly job of justifying this viewpoint.

One of the most useful functions of these studies is to serve as a check upon the facile generalizations so frequently made in standard medical histories. It is true that a distinction did exist between physicians and surgeons and that many physicians, as gentlemen and university products, considered surgery beneath them, but the better physicians recognized that the two went hand in hand—and many physicians practicing outside the major towns had no choice but to act as surgeons. According to Leo M. Zimmerman, even the celebrated physician William Harvey practiced surgery. Another assumption questioned is that Oxford and Cambridge provided only a theoretical education in medicine. A. H. T. Robb-Smith and H. M. Sinclair in their essays on Cambridge and Oxford clearly show that students did receive a measure of clinical training and that English physicians were at least as well trained as their European counterparts.

As with most books of this nature, the quality of the essays is uneven, there is a good deal of repetition, and not all areas have been covered. Nonetheless, this is a fine intensive study of a limited time period. Allen Debus deserves credit for bringing these papers to publication and for his own excellent essay on the impact of Paracelsian medicine.

JOHN DUFFY
*University of Maryland*

BRIAN MANNING. *The English People and the English Revolution, 1640–1649*. London: Heinemann. 1976. Pp. x, 390. £9.50.

Brian Manning focuses upon the role played by the "middle sort of people" in the English Civil War, and argues that it was their grievances against the ruling elite's exploitation of its political power to advance its economic ends that were the "main force behind the events" of these years. This thesis is sustained first by an exhaustive account of the agitation through which the Londoners successfully pressured the king and the Lords in 1641, and then by discussion of the agrarian disturbances which exploded in the provinces in the same period. A long section is devoted to the various manifestations of popular proparliamentarian activism at the outbreak of the struggle. Another section argues that the war was steadily transmuted into a class conflict as the "middle sort," hostile to the ruling classes who had flocked to the king as the bastion of their privileges, came to form the backbone of the Parliamentary party and, increasingly, to provide its leadership. The work concludes with an examination of the Levellers, who, in their onslaught upon the establishment and their constitutional program designed to "disassociate wealth from power," Manning argues, were the spokesmen for the "middle sort."

A number of Manning's specific suggestions in this work are most important contributions to the field. The argument that the growth of Royalist sentiment in 1641 owed more to conservatives' fears of the threat to the sociopolitical order posed by popular agitation than to principled objections to Pym's constitutional or religious policies is particularly compelling. Similarly, Manning's stress upon popular involvement at the outbreak of the war is a valuable corrective to the recent series of local studies which have concentrated upon the behavior of the county elites.

The overall thesis of the work, though most stimulating, is less convincing. It has two major weaknesses. First, as a category for social analysis, the "middle sort of people" is so broad as to be amorphous. It is hardly possible to attribute common grievances, let alone common aspirations, to a group whose defining characteristic is that its members were neither rentiers/financiers nor wage-earners, and which encompasses, *inter alia*, Lincolnshire fenmen, London Merchant Adventurers, and Birmingham metalworkers. This problem is compounded by Manning's virtually exclusive reliance upon literary sources. His arguments frequently rest upon the categories—"the better sort," "the chief of the town," "the vulgar"—employed by contemporary commentators whose social taxonomy was usually tendentious and, at best, lacked a common frame of reference. Second, Manning never demonstrates that the political objective, a "greater measure of democracy," which he attributes to the "middle sort of people," was espoused by more than a tiny minority of this group. The conclusion that the Levellers "articulated the aims and attitudes" of the "middle sort" both neglects those movements—the peace petitions in 1642–43, the Clubmen, the London Presbyterian organization—which suggest other popular aspirations and concerns, and minimizes the dif-

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ferences between the Levellers and the godly, for whom a secular democratic theory held few attractions.

CLIVE HOLMES
*Cornell University*

HILARY L. RUBINSTEIN. *Captain Luckless: James, First Duke of Hamilton, 1606–1649*. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. viii, 307. $15.00.

Posterity has not been kind to the First Duke of Hamilton. Many of his contemporaries viewed him as one of the major villains of the English Civil War, a man who deviously sought to betray Charles I under the guise of helping him and one who aimed at securing the crown for himself. Modern opinion remembers him as the ineffectual leader of the invading Scottish forces routed by Cromwell in the running battle of Preston. In this interesting and judiciously researched study, Rubinstein is concerned with a more rounded portrait. Her title is an indication that Hamilton's posthumous reputation is not wholly inaccurate, but she portrays him, on the whole, as maligned and misunderstood. There is no attempt to deny that Hamilton had inadequacies, both in the world of politics and on the field of battle, but Rubinstein clearly indicates that he was a well-intentioned man out of his depth in a complex political situation which he did not create and could not control.

As potentially the most influential nobleman in Scotland, Hamilton was a logical choice for the role of negotiator with the Covenanters. He did his best, as Rubinstein amply shows, but the sad fact was that those with whom he had to negotiate, especially Argyll, were simply cleverer than he was. His advice to the king that the Covenanters would stay neutral was not as ill-considered as events would seem to indicate; if the Royalists had achieved a quick and early victory, his assurances would have had some point. The achievement of negotiating the Engagement has been overshadowed by the disaster of Preston, but it was an achievement nevertheless.

On the surface, Hamilton is not very promising material for a biography, but Rubinstein's sympathetic portrait nicely fulfills its main intent of showing that he was important, even if inadequate. He had his virtues and these are clearly delineated; his shortcomings, notably his lack of quick wit and his fits of depression, are not ignored. In the end this excellent biography can only make one feel sorry for Hamilton, a man at sea in circumstances that made his shortcomings loom larger than his virtues.

ROGER HOWELL, JR.
*Bowdoin College*

JONATHAN MARWIL. *The Trials of Counsel: Francis Bacon in 1621*. Detroit: Wayne State University Press. 1976. Pp. 236. $14.95.

Bacon composed his *History of the Reign of Henry VII* in the summer of 1621 while he recovered from the shock of his impeachment and sought means to revive his fortune. Jonathan Marwil's intention is to explain why Bacon wrote such a work at the nadir of his public fortune and to see how it relates to his career as a whole. Thus his study covers far more than the circumstances of Bacon's fall and the actual composition of the work in 1621; more than a third of the text is devoted to a search for "pattern and style" in Bacon's development up to 1621.

Marwil has several key points that he wants to establish. Bacon, in his view, fell because of "temperament and a shaky status" more than because of misdeeds. As Lord Chancellor, Bacon yearned to make policy; Marwil shows clearly that in 1621 he was far from realizing that desire, and because of his continuing conviction that men could be persuaded "to perform the parts that his own self-interest assigned them," he lost even that modest grasp on policy. The desire to control policy, so marked in 1621, was, in Marwil's view, the predominant characteristic of Bacon throughout his public life. Marwil persuasively argues against the prevalent view of Bacon as a man torn between the contemplative and the active life. It was the view Bacon wanted posterity to have, but his assessments of himself must inevitably be suspect; his basic ambition was always political. And this is precisely where *The History of Henry VII* fits in. Like many of his other writings, it enlarges his experiences and perceptions into general rules. It is, as Marwil demonstrates, a book about kingship, not about England; it is history designed to teach a lesson, not just to record the past, and in it Henry both as man and king "comes close to being a mirror image of Bacon's talents, ideas, and personality." Marwil's analysis of the *History* shows how much Bacon's achievement sprang from his skills as a rhetorician; his placing of the *History* in the context of Bacon's career builds a good case that the Lord Chancellor's ultimate failure was, in important ways, the result of his being a prisoner of that same talent.

ROGER HOWELL, JR.
*Bowdoin College*

JOHN CHILDS. *The Army of Charles II*. (Studies in Social History.) Buffalo: University of Toronto Press. 1976. Pp. xii, 304. $25.00.

John Childs is concerned with three different aspects of the army of Charles II—the army as a

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social institution, the army as an instrument of force, and the army as a subject of royal-parliamentary debate. Childs first studies the origins, composition, religious affiliation, conditions of life, discipline, pay, administration, and employment of Charles' army. He then shows us the army outside the settled conditions of England, describing units stationed in Tangier and the Western Hemisphere, those attached to the Portuguese and French armies, and others that served as an expeditionary corps in Flanders. After a brief discussion of the Scottish and Irish establishments Childs concludes by detailing Parliament's fears of a standing army.

Childs' book is considerably more professional than the army he has studied. The book is disciplined, reliable, and even attractive; it merits a kinder fate than that of the abused soldiers with whom it deals so sympathetically. I suspect, however, that the majority of Childs' audience will consist of specialists in Restoration England. There are a couple of inherent drawbacks in his subject which will diminish its broader appeal. One is the scarcity of material on the social background and conditions (indeed, even the uniforms) of the common soldier. Childs' evidence thus is heavily weighted in favor of the officers of Charles' army. A second drawback lies in the limited contribution made by Charles' army to the evolution of the British army. True, Charles may have had England's first regular peacetime army, but it was generally poorly led and highly imitative of French models. Compared to developments in the France of Louvois and Vauban, English military developments under Charles are pretty bland fare. Childs himself admits, "The cream of the armies which won glory for Marlborough was taught the art of warfare in mercenary formations under foreign governments." Childs is an imaginative historian and a competent writer, but I am afraid the attention of military historians will not easily be drawn from the armies of Cromwell and Marlborough.

JONATHAN R. DULL
*University of Texas, Austin*

BRYAN LITTLE. *Sir Christopher Wren: A Historical Biography*. London: Robert Hale. 1975. Pp. 288. £6.00.

*Lector, si monumentum requiris circumspice.* The famous epitaph on Sir Christopher Wren's grave in St. Paul's may justly be thought to invite the reader to look around not only at the immense domed space of the cathedral but at the whole of England's great metropolis and beyond. For, if the most magnificent achievement of the architect's long and fruitful career is St. Paul's, his most satisfying works are the City churches, the Royal Hospital at Greenwich, and the brilliant buildings at Oxford and Cambridge.

Genius, longevity, the right social, religious, and political connections, and the Great Fire all conspired in Wren's favor. It is this sense of interconnection that most fascinates Bryan Little, and, accordingly, he pays very close attention to the whole range of Wren's interests, connections, and duties. Thus, for example, while work was proceeding in 1689–1691 on his major redesign of Hampton Court, Wren, as Surveyor General of the Royal Works, was also involved in a host of lesser projects: a report on the upkeep of the waterfowl in St. James's Park, the erection of royal icehouses, repairs on the harbor works at Margate, the refurbishing of several cells in the Tower, and so on. The problem here is that Little does not always make it clear to what extent Wren, as opposed to his underlings, was directly engaged in these matters; the effect is virtually to swamp the architect—and his biographer—in details of questionable significance.

We learn the names of master masons, injured workmen, Oxford dons; the cost of Portland stone, wood carving, plasterwork. All well and good, but we end with surprisingly little grasp of Wren and his period. What is the significance esthetically or historically of Wren's career? What did his contemporaries or, for that matter, the architect himself think he was accomplishing? How shall we interpret the fact that Wren apparently showed little interest in visiting the sites of many of his projects, that he wished to demolish Tudor Hampton Court and mask Inigo Jones's buildings at Whitehall and Greenwich, that he had no hesitation in moving or reorienting ancient churches? Does Wren's career mark an important shift in the relationship of man and his environment, a diminution in the sense of human rootedness, a desacramentalization of space? These and similar questions lie disappointingly far away from Little's "historical biography."

STEPHEN J. GREENBLATT
*University of California, Berkeley*

HENRY L. SNYDER, editor. *The Marlborough-Godolphin Correspondence*, in three volumes. New York: Oxford University Press. 1975. Pp. xxxix, 588; 589–1185; 1187–1794. $125.00, the set.

One cannot in a brief review begin to spell out the many fresh insights into government and politics in Queen Anne's day that are the fruits of Henry L. Snyder's labors; he makes available the entire Marlborough-Godolphin correspondence (1785 pieces—383 previously published in full and 497 in

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part). Certainly, the editor is more than justified in asserting that "this correspondence is . . . an indispensable source for the history of England" between 1702 and 1710.

In general, Snyder's editorial work is most helpful: many undated and misdated letters have been placed in correct sequence; lacunae (along with some errors by earlier editors and biographers) in the unravelling of the ciphers have, for the most part, been filled; and the notes aid considerably in elucidating the text as well as in identifying a host of persons and places. Slips, either in typesetting or in the editorial apparatus, are few, and the index appears quite thorough.

Clarendon Press is to be commended for undertaking this project, and all that we can ask now of the editor is that he bring to culmination both this and his previous work (a dozen of his articles appear in the citations) with a full-scale study of the Marlborough-Godolphin administration.

HENRY HORWITZ
*University of Iowa*

OLWEN HEADLEY. *Queen Charlotte*. London: John Murray; distributed by Transatlantic Arts, Levittown, N.Y. 1976. Pp. xiii, 371. $15.00.

Queen Charlotte was not particularly interesting or important, and my first reaction to this fine monograph was that much scholarship had been lavished on a nonsubject. My second reaction is that it was worth doing. Olwen Headley, who was assistant in the Royal Library, Windsor Castle, knows the sources well. She uses them skillfully to present a portrait of George III's queen which is a bit too sympathetic, but judicious, consistent, and rich in detail. This volume and recent biographies of George III by John Brooke and Stanley Ayling provide an understanding of the royal couple which, perhaps, is more favorable than they deserve, but which is certainly closer to the truth and better rooted in the sources than our previous view.

The role of the queen consort in eighteenth-century politics deserves attention. Mary II, Queen Caroline, and Queen Charlotte were actually or potentially of political importance. The queen's household was significant for its patronage, which was considerable, and also for the political benefits which might be derived from proximity to the throne. George III insisted that Queen Charlotte be nonpolitical, but when his mental illness struck, the political potential of the queen immediately appeared. Headley touches lightly upon this subject. The queen was also important as a social leader. George III and Queen Charlotte exercised an important influence upon moral values, recreations, and tastes in the fine and domestic arts. Headley's coverage of this aspect of the queen's role could scarcely be improved.

The book does not help much with the vexing questions which still surround the king's mental illness. Headley handles this subject without mentioning the current code word, porphyria. I like that. In my judgment, historians can describe the king's symptoms and ponder the consequences of his affliction, but should not attempt a diagnosis, although I suspect the psychohistorians will soon be back for another crack at it. George III's ravings revealed deep-seated problems in his relationship with his queen; Headley presents the information, and leaves it at that. Queen Charlotte may have been part of the problem, and her inadequacy in dealing with the king's illness may have made it worse. On that question Headley presents useful evidence, but draws no conclusions.

The main value of this book is in its scholarly picture of the life of a queen. Much of the volume is taken up with the long succession of banalities which comprised the life of Queen Charlotte: marriage, pregnancies, wet nurses, dry nurses, cribs (with or without rockers), raising and marrying children, household finances, clothing, jewels, furniture, decorating and redecorating palaces, trips to the seaside, and the fate of foreign relatives. Much of the time and energy of crowned heads, courtiers, and politicians was absorbed in that sort of thing. If Headley's meticulous scholarship helps us to understand the forces tempting George III into a life of empty formalities and interesting hobbies, we can appreciate even more those times in his long reign when he truly tried to "be a king."

E. A. REITAN
*Illinois State University*

G. J. MARCUS. *Heart of Oak: A Survey of British Sea Power in the Georgian Era*. New York: Oxford University Press. 1975. Pp. xi, 308. $27.50.

G. J. Marcus brings a practiced pen to his latest offering, for he has written a number of books and articles on the British navy. The period covered in the present volume is roughly from the mid-eighteenth century to Napoleon's fall in 1815. This period represents the last time the navy was engaged in fairly continuous combat for mastery of the seas. It was no accident that the famous *Rule Britannia* was written at this time. Never before or since would England contest with her continental adversaries for such vast areas of the globe.

The author divides his work into twenty chapters, each treating an aspect of the larger picture. The chapters "Strategy and Tactics," "Seamanship and Navigation," "Discipline and Morale," "Sweethearts and Wives," etc. are by no

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means definitive, but are rather a good sampling of the elements comprising eighteenth-century sea power. Both innovations and archaic anomalies were rife in the navy at this time. On the one hand all ships were copper-bottomed to impede sea growths and increase speed; on the other hand no one was safe from the press gangs. The Common Law was swept aside, and farm boys and clerks—virtually every working man—were eligible for impressment. There were 145,000 men in the navy in 1812. The wonder was that there were so few mutinies. One of the chief reasons for this, of course, was the ubiquitous discipline. Marcus gives a vivid illustration of a man who had attempted desertion and was "flogged round the fleet," twenty-five lashes alongside each ship in harbor. But it was no time for constitutional niceties, when few would enlist and Britain was threatened with invasion at any moment.

The quality of French and Spanish warships was often superior to their English counterparts, which is understandable when one realizes that Britain kept hundreds of ships at sea, blockading, convoying, and fighting. *They* turned out ships like sausages! Enemy tactics and signalling were also often much superior. But in seamanship—staying off the French ports in all weather, defeating their opponents in a gale, and bringing the enemy to action at night—the British were without equal. The dockyards and port towns are not neglected. Portsmouth, the chief naval base during these memorable years, is realistically depicted.

A survey volume of this nature scarcely requires archival material, and it is largely absent. It is furnished, however, with a good annotated bibliography, a complete index, and a useful glossary of naval terms. One would expect some colored illustrations for its $27.00 price, but these are not to be found, although a good selection of black-and-white prints is included.

This book ably captures the energy and enthusiasm of Georgian Britain in all its naval endeavors.

REGIS A COURTEMANCHE
*C. W. Post College*

E. A. SMITH. *Whig Principles and Party Politics: Earl Fitzwilliam and the Whig Party, 1748–1833*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. xiii, 411. $25.00.

E. A. Smith contends in this well-researched and well-written volume that the Whigs possessed a distinctive creed to which the party adhered in opposition and which it tried to translate into practice when in office. The survival and transmission of these principles is approached through the life of William, Earl Fitzwilliam, who, it is contended, was "one of the leading and . . . most significant of the men who acted as transmitters of that political heritage from the days of Rockingham to the period of the Reform Act." Fitzwilliam was Rockingham's nephew and in 1782 inherited his estates and part of his political influence.

That the Whigs had principles can hardly be doubted, and it would appear that their aristocratic conception of the social order and its reflection in government was unique to them. Burke spoke for the values of the society, not for party alone. Smith explicates well the Burkean definition of cooperative social and political relationships—benevolent paternalism and grateful deference—without, however, adequately weighing the reality of the economic power which could be bared as necessary before, during, or after elections by Fitzwilliam or others of his class.

Smith sees in the long life of Fitzwilliam the epitome of eighteenth-century Whig principles and values. He may have become a symbol of values, but on other levels he was not very effective. He was rarely, if ever, a national leader. He was in cabinet office a total of only eighteen months and was Lord Lieutenant of Ireland less than three months. He was a northern Whig grandee who preferred local affairs and who shrank from responsible political office at the center. The Whig party never looked solely or largely to him for direction or leadership. His political ideas appear mostly instinctive, and his intellectual equipment was sorely limited. He had next to no ability to change his ideas in the light of changed circumstances, a mark of mature political leadership. He had a fixed idea in favor of Catholic emancipation, but his fear of parliamentary reform was so great that he was unable even to consider it.

The book is good political history and fills a real need for a study of the condition and situations of the Whig factions in the difficult and confusing period of Pitt's leadership, in the long years of war against revolutionary and Napoleonic France, and in the war's aftermath. The personal and political relationships of Fitzwilliam, Charles James Fox, Burke, and Portland make poignant reading and reflect faithfully the issues and tensions of the times. Particularly valuable is Smith's explanation of the political background, both English and Irish, of Fitzwilliam's ill-fated Irish Lord Lieutenancy. The Duke of Portland is not spared his great share of blame for its failure. However, was the dismissal of Fitzwilliam from his Yorkshire Lord Lieutenancy after his censure of the magistrates following Peterloo a martyrdom, when the action arose principally from calculated political motives? To picture him as a civil libertarian in the light of his attitudes in 1795 and 1812 requires one to push Whig principles a bit too far.

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The volume is a valuable contribution to a fuller understanding of the part played by the Whigs in the political process in late Georgian England.

RAY A. KELCH
*San Francisco State University*

JOHN STEVENSON and ROLAND QUINAULT, editors. *Popular Protest and Public Order: Six Studies in British History, 1790–1920*. New York: St. Martin's Press. 1975. Pp. 242. $16.95.

DAVID JONES. *Chartism and the Chartists*. New York: St. Martin's Press. 1975. Pp. 229. $14.95.

The *grand peur* of British publishers, which began c. 1973 and shows no signs of abating, may prove to have dealt a death blow to one of the major growth areas of academic output—the volume of collected essays. The collection edited by Stevenson and Quinault serves to remind us that if this happens we will be missing something worthwhile. Though ranging over more than a century, its contributions are well ordered and internally coherent. With the exception of an impressive short piece on the Plug Riots of 1842 by F. C. Mather, the contributions come from the younger generation of British social historians and show that the new areas pioneered in the 1960s by George Rudé, E. P. Thompson, and Eric Hobsbawm are being profitably developed. D. Philips presents a careful and useful study of rioting in South Staffordshire, a teeming area of domestic outworkers, mining, and iron and steel manufacture, which has been somewhat neglected. Eric Richards offers a clear-headed and notably unsentimental account of riots in the Scottish Highlands. He finds, not surprisingly, that most Highland resistance was the unprepared reaction of desperation. Roland Quinault throws new and much-needed light on the nineteenth-century justice of the peace—the basic unit of public order in the English countryside. His study of the Warwickshire bench in mid-century shows the declining importance of parson magistrates and also the growing strength of the aristocracy and larger landowners as active magistrates. As in the eighteenth century, social and judicial functions were closely intertwined.

It is a pity that the piece in this collection which will be most used by undergraduates—John Stevenson's on food riots—is in general the most disappointing. Stevenson concentrates on regional variations and uses his knowledge of the communications network to good effect to show that riots occurred most frequently up to 1800 at nodal distribution points and in the growing manufacturing towns. The problem, however, is that both Stevenson's list of food riots and his analysis of their geographical distribution are seriously deficient. In particular, he grossly underestimates the extent and importance of rioting in the northwest. Apart from his material on communications, he offers little that takes the student beyond Rudé. More detailed studies are needed before a short, integrated explanatory piece of the kind Stevenson attempts can come off. Here he offers too many hostages to fortune.

To this collection the editors offer a useful introduction on the mechanics of protest and riot. It does not entirely avoid banal observations, for example when discussing why near-riots did not become riots proper, but it attempts to fill a yawning gap in the literature, and a partial success is decidedly better than no attempt at all. The introduction, quite unlike the remainder of the collection, is inadequately footnoted. Little has been written on the methodology of rioting, but not as little as is indicated here.

Chartism is, of course, the most celebrated movement of popular protest in nineteenth-century Britain, and David Jones offers yet another general study. It does not attempt to provide a narrative history, and students are wise to approach it with the broad landmarks already in their minds. Nor is it a great contribution to original research. Instead, Jones makes mature use of the mass of secondary literature in the field and provides his own insights from time to time. He is particularly good at indicating where future research might profitably concentrate, and while one might question his judgments from time to time, as on the extent to which O'Connor gave impetus to the movement in the 1840s, Jones has provided a lucid and worthwhile brief account. We still await, however, a definitive modern study of Chartism. Is it too much to hope that the family which provided the seminal study of the emergence of working-class consciousness will soon give us a new interpretation of its most formidable political expression?

ERIC J. EVANS
*University of Lancaster*

R. G. KIRBY and A. E. MUSSON. *The Voice of the People: John Doherty, 1798–1854, Trade Unionist, Radical and Factory Reformer*. Manchester: Manchester University Press. 1976; distributed by Rowman and Littlefield, Totowa, N.J. Pp. ix, 474. $30.00.

John Doherty, a Belfast district cotton spinner, moved to Manchester in 1816 and went on to become England's most influential working-class leader in the 1820s and 1830s. This first biography of Doherty makes an important contribution to our understanding of early trade-union activities. As there are no surviving Doherty papers, R. G. Kirby and A. E. Musson have used newspapers, journals, pamphlets, Home Office documents, and

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the Francis Place manuscripts in the British Museum to good advantage. This extensive research has enabled the authors to correct some errors about Doherty and trade unionism that have existed since the Webbs and Hammonds. Their work updates G. D. H. Cole's essay on early trade-union activity first published in 1939 and republished in 1952 as *Attempts at General Union: A Study in British Trade Unionism History 1818–1834*. In addition to providing an account of Doherty's efforts at union organization, the authors discuss his journalistic undertakings, his work on behalf of factory reform, and his ventures into radical politics.

Doherty was the moving force behind the creation of the Grand General Union of Cotton Spinners in 1829, but his ultimate objective was to convince local labor leaders that their interests were best advanced by a well-disciplined national union. Hence, Doherty attempted (also in 1829) to build an effective union of all the trades through the National Association for the Protection of Labour. Within two years the Grand General Union and the N. A. P. L. fell victim to lack of cohesion, powerful manufacturing interests, the anti-trade union press, and Doherty's serious shortcomings as a leader.

The authors are at pains to show, contrary to Cole, E. P. Thompson, and J. F. C. Harrison, that Doherty was not captivated by the ideas of Robert Owen. Indeed, one of the major themes of this gracefully written biography is that the plan for a national union did not come from any "Owenite explosion" among the workers between 1829 and 1834. Though the authors are not totally convincing on this point, the arguments advanced are stimulating.

RONALD K. HUCH
*University of Minnesota, Duluth*

M. C. REED. *Investment in Railways in Britain, 1820–1844: A Study in the Development of the Capital Market*. (Oxford Historical Monographs.) New York: Oxford University Press. 1975. Pp. xiii, 315. $20.75.

The phrase-turner in economic history, yearning for romance, likes to play with big pieces of capital equipment—steam engines, Bessemer furnaces, and railways. Over the past decade or so quantitative economic historians have in their dismal way been spoiling the fun by importing instruments of measurement into the playground. M. C. Reed's book, drawing with exemplary thoroughness detailed lists of holders of stocks and bonds from the records of the companies, their agents, and their parliamentary regulators, measures the locations and trades of early financiers, from widows to dukes and from Londoners to Mancunians. Though he provides accounts of the financial events and institutions of the early railway age, the author's chief task is to collect the numbers, a task too often neglected by other exponents of the "new" economic history in favor of insubstantial theorizing and by exponents of the "old" in favor of inconclusive tale-telling.

The rewards of virtue are two new facts. First, investors with no direct interest in the neighborhood of a new railway line and employed in commerce rather than in landowning or manufacturing were from the beginning dominant: the capital market was national and impersonal. This fact contrasts with Fishlow's finding that in the United States landowners along a route responded to the "signals of social gain" by investing heavily in it and with Clapham's belief that early on "local patriotism" rather than "blind capital seeking its 5 per cent" supported railways. Second, "national" is not merely an elegant variation on a London market for railway (and other) shares. Indeed, after the floating of railway shares had created a market—a subtheme of the book being that railways gave birth to provincial markets as government finance had given birth to the London market—Lancashire in general and Liverpool in particular invested more in railways than did London. This second fact accords with a growing realization in the field that the machinery for mobilizing provincial saving was sophisticated early in Britain's industrialization.

These important findings, sad to say, are obscured by the style and organization of the book, whose overinclusiveness and tedium betray its origin as a thesis for the D. Phil. Further, the energy employed in compiling the statistics is not always matched by subtlety in using them: at the most elementary and significant level Reed does not add together the statistics for each line or crossclassify investors simultaneously by occupation and residence. Still, if the book is not a masterpiece of either literary or statistical history, it is nonetheless a step in the quantitative rebuilding of British economic history.

DONALD N. MCCLOSKEY
*University of Chicago*

M. J. CULLEN. *The Statistical Movement in Early Victorian Britain: The Foundations of Empirical Social Research*. New York: Harvester Press Limited/Barnes and Noble. 1975. Pp. xii, 205. $22.50.

Much of our view of nineteenth-century Britain is shaped by government statistics and reports and the work of statistical societies. M. J. Cullen points out that the individuals who created these lenses through which we view nineteenth-century Britain

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ground them to fit the perspective of their own vision, not as microscopes or magnifying lenses created with accuracy as a prime concern. These early statisticians, many of them involved in the collection of both government and private statistics, confused propaganda and fact, tending to shape their research to bear out their own prejudices and opinions. For example, they were convinced that urbanization, not industrialization, was the cause of Britain's ills and determined to show that public health and education were the central issues in reform. Their concern was less with presenting accurate statistics as the raw material for policy making than with shaping data collection for a particular purpose: "by an individual, corporate, and governmental attack on the urban environment the statisticians expected to see the creation of the preconditions for a more moral society." The confusion of fact with propaganda is well illustrated with the case of Edwin Chadwick, who appears here less as a giant of the reform movement than as the epitome of the faults of contemporary statisticians. He was methodologically suspect, often stubbornly wrongheaded, and determined to verify the validity of his prejudices.

The short biographies of the leading men of the statistical movement, presumably designed to suggest the connection between personal experience or political ambition and the desire for reform, are often weak and tend to raise more questions than they answer. For example, we are told that William Farr was "probably . . . not very successful" in medical practice, but we are not told why this assumption is made; the description of Henry Marshall's father as "a relatively poor" man is irritatingly inadequate. There are occasional lapses into detail for its own sake which make the book dull in parts.

Although the author notes that this is not an introduction to the statistical sources of the period, he enhances its usefulness by pointing out some of the major areas of error and suggesting some of the means by which the errors may be corrected. This is a useful analysis of the statistical movement and the statisticians on whom historians have depended for much of their material, as well as a necessary corrective which should be read by students before they are turned loose on the blue books.

JANET ROEBUCK
*University of New Mexico*

RICHARD L. BLANCO. *Wellington's Surgeon General: Sir James McGrigor*. Durham: Duke University Press. 1974. Pp. xiv, 235. $9.75.

This first biography of the "Father of the Royal Army Medical Corps" is a solidly documented account of British military medicine from 1750 to 1850, with discussions of civilian medicine and the campaigns of the army, enhanced by good maps and photographs. James McGrigor purchased a commission in 1793 at the age of 22, served in several overseas campaigns and at garrison posts in England, and was an experienced military surgeon when he went to the Peninsula in 1812 as Wellington's Inspector of Hospitals.

He brought vigor, common sense, and organization to the medical system. He won the confidence of Wellington—one of the first generals to appreciate that medical care could help keep an army in the field; re-established the regimental hospital; installed an ambulance and evacuation system; and argued for better rations and warmer clothes for the troops. He developed a morbidity-mortality reporting system to provide timely and accurate health data. It is fair to say that his conservation of manpower was an essential contribution to Wellington's victory.

McGrigor became director general of the medical department in 1815, just before Waterloo. His thirty-six-year period as director general is presented in a single chapter and emphasizes that low budgets, army indifference, and public apathy blocked McGrigor's plans to improve the medical department. He established a health report system, raised the professional standards for medical officers, and built a library and pathology museum. But it was the medical department he built and the army that Wellington controlled for nearly forty years that went unprepared to the medical and military disasters of the Crimea in 1854. If it is the function of an Army in peacetime to train and prepare for war, then neither man did his job.

James McGrigor was a good surgeon and soldier who did his best for his country. How well did the author do by McGrigor? Richard L. Blanco often uses twentieth-century medicine to judge eighteenth- and nineteenth-century practice, as if he faults McGrigor and his contemporaries for not knowing of the germ theory of disease; or of pathophysiologic correlation of sign, symptom, and organ damage; or of vector transmission of disease, and so on. There are some errors of medical fact: there is no proof of the superiority of medical care in the Navy; rats have nothing to do with epidemic typhus; the epidemiology of plague was not known in 1802; gangrene cannot spread through regiments; the tourniquet was not a new technique in 1814; and virology could not "advance" in the 1800s because the very concept of viral disease was unknown.

Blanco understands the essential difference between military medicine and civilian medicine. Military medicine is a system for the delivery of

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health care in mass casualty situations and for applying preventive medicine and public health techniques to prevent or reduce the incident of disease in armies. The direct care of individual patients is very similar to that of civilian life. In war, the success of military medicine depends directly upon support from the battle commander and upon his confidence in his medical staff and its advice. I wish the author had explored why McGrigor got on so well with his commanders, and how McGrigor acquired and defined the concepts that led to his success in the Peninsula. One wonders why his excellent system of 1814 was unavailable at Waterloo in 1815. I finished this biography still wanting to know James McGrigor. I wish more of his autobiography, letters, and official correspondence had been quoted, rather than excerpted or paraphrased, and that the man, not his times, had been the focus of the author's interest. Still, it is easy to ask for the book the author did not write.

In a superb bibliographic essay, Blanco notes the lack of an Army Medical Department equivalent to the Naval medical history by Keevil, Lloyd, and Coulter. Sir Neil Cantlie's excellent *A History of the Army Medical Department* (1974) has since been published. Blanco should be read as the best survey of the period, must be used as an example of how to integrate military and medical history, and must be consulted for the wealth of reference material. Cantlie is the encyclopedic source.

ROBERT J. T. JOY
*Walter Reed Army Institute of Research*

JAMES WINTER. *Robert Lowe*. Buffalo: University of Toronto Press. 1976. Pp. xiv, 368. $17.50.

This sympathetic reappraisal of Robert Lowe, first Viscount Sherbrooke, takes account of such recent scholarship as Ruth Knight's study of Lowe's Australian career and the works of M. Cowling and F. B. Smith on the Second Reform Act. The focus is firmly on Lowe, that incredibly self-disciplined albino whose academic triumphs, despite near blindness, were as astonishing as his ferociously impolitic judgments. James Winter argues that a high-principled consistency makes sense of the superficially inconsistent positions Lowe took while in Australia. Indeed, the clue to understanding Lowe is his devotion to principles, all of them firmly anchored to the truths of classical economics. His sort of Liberalism is aimed at efficiency and economy, not democracy, in government. Educational reform was vital to establishing a "meritocracy," itself the more necessary when the Reform Act of 1867 opened the door to democracy. If he never actually said, "We must educate our masters" (p. 187), he firmly believed that an educated elite was essential to good government.

The author is persuasive in defending Lowe the educational reformer from Matthew Arnold's contemporary attacks, but the man who comes to life in these pages was surely one for whom the boundary between principled conviction and sheer self-deception was but feebly guarded. This does not mean that the author fails in his objective: to produce an improvement over the officially commissioned biography which appeared in 1893. Notes and bibliography indicate the solid foundations of research on which this useful contribution rests. But there are blemishes. It is confusing to read on page 133 that Lowe quit his job at the Privy Council without learning until page 150 what the job was. There are baffling phrases like "The judaizing cast of mind" (p. 166), which Lowe and Arnold agreed was a barrier to critical intelligence! Finally, the proofreading is far below the standard expected of a university press. Dozens of errors distract the reader, and proper names are especially vulnerable: Macaulay is all right in the text, but not in the index, while Chichester Fortescue (in the index) pops up as Fortesque in the text.

BARRY MCGILL
*Oberlin College*

ERIC TRUDGILL. *Madonnas and Magdalens: The Origins and Development of Victorian Sexual Attitudes*. New York: Holmes and Meier. 1976. Pp. xii, 336. $15.00.

Eric Trudgill illuminates the fascinating world of Victorian sexuality by examining the origins and development of middle-class sexual attitudes from the mid-eighteenth to the early twentieth century. Reminding his readers that the Victorian middle class was not a monolith and that it cannot be generally condemned as hypocritical, he carefully explains the sources of sexual attitudes and the reasons behind the paradoxical variety and dissonance of sexual thought and conduct. Among his conclusions is the assertion that Victorian men were not so much hypocrites as casualties of a prudish ideal which many sincerely believed in, but simply could not live up to.

As the title indicates, the book deals primarily with Victorian women, respectable and otherwise. The author traces the evolution of the position of women and of attitudes toward them in life and literature. By analyzing the development of the ideal home and of the ideal woman as its guardian angel in connection with religious, political, economic, and social problems, Trudgill provides new insights into the premium which was placed on feminine purity.

His exploration of prostitution, the "great social evil," is similarly revealing. The author examines the causes and consequences of prostitution, the ever-changing social and literary attitudes toward magdalens, and the vicissitudes of reclamation ef-

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forts. He argues convincingly that the Victorian code of morality was based on the paradoxical partnership of marriage and prostitution. While the prostitute was the enemy of sexual purity, she was also its ally, for she provided an outlet for men who could not fulfill the ideal of marriage, but who nevertheless sought to maintain the sanctity of their own homes.

*Madonnas and Magdalens* is a meticulously researched and realistic book which makes a valuable contribution to an important and absorbing aspect of English social history. The author demonstrates admirable analytical ability, a willingness to draw original conclusions, and a remarkable comprehension of his subject. His sympathetic and fair treatment of Victorians of both sexes has convinced this reviewer that, as Trudgill argues, the Victorians deserve much greater understanding than they usually receive.

Readers will regret an unusual and inconsistent form of citing references as well as the occasional coining of phrases which tend to obscure rather than clarify meaning. However, these are minor flaws in a generally well conceived and executed work.

KATHLEEN E. MCCRONE
*University of Windsor*

PATRICIA BRANCA. *Silent Sisterhood: Middle Class Women in the Victorian Home.* Pittsburgh: Carnegie-Mellon University Press. 1975. Pp. 170. $12.50.

The theory of modernization enjoys considerable popularity among social scientists. In *Silent Sisterhood* Patricia Branca has used this theory to re-evaluate the lives of Victorian middle-class women. Rather than being the idle, frivolous prude of the conventional stereotype, Branca's middle-class Victorian wife is the first modern woman. If the stereotype reflected any reality at all, it was the lives of upper- and upper-middle class women, not the majority of the middle classes whose incomes ranged from £100 to £300 throughout the Victorian period. In those families wives managed the household budget and undertook the physical labor of housework and childcare, perhaps assisted by one servant. They became astute consumers of new household equipment and critics of prevailing standards of health care for themselves and their children. They opposed fatalistic attitudes toward illness and looked increasingly for "scientific" aid in combating it. Ironically, the doctors whom they consulted often added to the dangers of infection by their unhygienic practices and their opposition to newly discovered methods for preventing the spread of disease. Middle-class women sought aid also from other sources, such as medical and household manuals, and from the purchase of patent medicines and contraceptives. Despite the contrary claims of some medical writers, Victorian middle-class wives enjoyed sex and sought to avoid the dangers of childbirth by limiting the number of their pregnancies.

Branca's thesis calls for validation. In *Silent Sisterhood* a small number of data support a large burden of generalization; this is a drawback for the reader, who cannot readily verify Branca's claims. Despite her use of statistics from London generally, and from Camberwell in particular, Branca relies heavily upon her own *a priori* assumptions about the modernization of Victorian middle-class women for her interpretations. She recognizes more readily the prescriptive nature of household than of medical manuals. Since physicians had good reason to persuade middle-class women to call the doctor more often than they did, one may well doubt Branca's assumption that medical manuals describe actual practices with precision.

Lastly, one has to question the power of any sociological generalization to explain the complexities of social history, expecially the power of modernization theory to explain the lives of middle-class women in Victorian England. The very richness and multiplicity of women's attitudes and experiences suggests that further research in women's history will serve to undermine the theory of modernization as applied to women's lives. For the further exploration of this issue, Branca's book provides a thought-provoking base.

JOAN N. BURSTYN
*Douglass College, Rutgers University*

JOHN S. GALBRAITH. *Crown and Charter: The Early Years of the British South Africa Company.* (Perspectives on Southern Africa, number 14.). Berkeley and Los Angeles: University of California Press. 1974. Pp. xi, 354. $12.75.

In this important new study of the formation and early years of the British South Africa Company, John S. Galbraith cuts Cecil Rhodes down to size—as one among a number of men who developed the company and as a man who combined ruthlessness and profiteering with imperialism. Galbraith also emphasizes that Rhodes was practically unrestricted by his board of directors or by the imperial government. In this closing words, "Between 1889 and 1896 the chartered company was an engine without a governor." This was due to Rhodes' power and personality and to the fact that other Britons also wished to assert British influence in northern South Africa. Galbraith tells how Rhodes and London business interests came together and subsequently won a government charter. Also included are descriptions of concession-hunting expeditions into the north, ventures noted for greed and duplicity. The desire for

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a charter was meant in part to overcome the deficiencies of such concessions and to end competing claims. For the government a chartered company was an inexpensive way of asserting British influence and preventing Transvaal, Portuguese, and German expansion in the area.

Galbraith shows convincingly that in this process there was no real understanding of the African societies involved; English interests always were dominant. He also shows that African leaders understood the situation on their home grounds and sought unsuccessfully to balance internal and external pressures. It is not a pretty story, and Galbraith lays it out with rich detail. He is well equipped for such work, having previously examined both British expansion in South Africa and other British chartered companies. For this study he has drawn most extensively on the company records in the National Archives of Rhodesia.

The major criticism I have of the book is that it sometimes leaves the reader uncertain about general patterns. For example, I wish that in the introduction Galbraith had provided a systematic outline of the African peoples involved, that later on he had explained the processes of company stockjobbing, and that he had concluded with his own close analysis of Rhodes' personality. It would also be helpful if the maps were more adequate for illustrating textual details. Nonetheless, those of us who work in imperial history will find this a major resource, especially for the study of ventures into "Rhodesia" and for the story of relations between government officials and the company.

DEWITT C. ELLINWOOD, JR.
*State University of New York, Albany*

BYRON FARWELL. *The Great Anglo-Boer War*. New York: Harper and Row. 1976. Pp. xiv, 495. $16.95.

Byron Farwell has put together a most interesting and absorbing account of the Anglo-Boer war. There is a grand sweep to it, innumerable battles described and integrated into a moving portrait of two cultures, as well as two peoples, at war. *The Great Anglo-Boer War* is far more substantial and important than the author's earlier *Prisoners of the Mahdi* and *Queen Victoria's Little Wars,* and Farwell is extremely skillful in deftly sketching the foibles and mannerisms of the various generals. General Methuen, for example, "found it easier to be gallant than to be wise," and General Warren's "preoccupation with oxen" indicates his defects as a general. This emphasis on the personal and often the humorous enables Farwell to cover the action of many battles and skirmishes without losing the reader's attention.

There are a number of shortcomings to the work, however. The author seems oblivious to the pejorative overtones of many of his terms: Africans are referred to as "Bantu," and the Khoi-Khoi and San people are presented as "Bushmen" and "Hottentots." And his conclusions concerning the bitter peace of Vereenig completely overlook the extent to which the British were prepared to sacrifice African interests in order to placate the defeated Boers, thus planting the seeds of the future racial clash still facing South Africa today.

Also, the work generally could have used many more footnotes. Even accepting the fact that it was meant to have a general, nonscholarly audience, many assertions need sources. Many readers, for example, would like to know Farwell's basis for the statement, "More Boer boys and girls under the age of sixteen died in British concentration camps than all the fighting men killed by bullets and shells on both sides in the course of the entire war."

Finally, in analyzing the slide toward war in the late 1890s, Farwell completely ignores the economic dimensions of the clash. Great emphasis is placed on history, culture, and personality, but virtually none on the economic reasons why Great Britain felt it had to control South Africa, or the economic motivations that were involved in the clash between the Boers and the Uitlanders.

On balance, however, *The Great Anglo-Boer War* is an engrossing and well-written account of an important conflict.

CHRISTIAN P. POTHOLM
*Bowdoin College*

C. J. WRIGLEY. *David Lloyd George and the British Labour Movement: Peace and War*. New York: Barnes and Noble. 1976. Pp. x, 298. $15.00.

This book is another product of the current boom in Lloyd George scholarship. Unfortunately it delivers considerably less than its title promises. Instead of delving into the ambiguous and hitherto unexplored relationship between that redoubtable politician and the British labor movement, C. J. Wrigley concentrates on the government's industrial policy during the First World War, the gradual extension of state control over manpower and production. Since much of this has been analyzed before, most notably by S. J. Hurwitz a generation ago and more recently by Arthur Marwick, this study, however well documented, merely confirms a familiar picture.

In a useful introductory section the author portrays Lloyd George as a conventional rural radical, preoccupied with land reform, temperance, and Welsh disestablishment and lacking real affinity with urban working-class aspirations. His emer-

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gence as a social reformer during the Edwardian period stemmed from a mixture of sympathy—emotional, not ideological—for the underdog and political sagacity. His radical rhetoric and growing reputation earned the confidence of organized labor, thus facilitating his successful mediation of industrial disputes.

The policy which Lloyd George termed "conciliation at first, but, failing that, the steam roller" summarizes succinctly the government's approach to mobilizing the labor force during the war. Wrigley observes that the much-vaunted Treasury Agreement of March 1915, based on voluntary relaxation of trade practices in wartime, was ineffective because the government was so blatantly one-sided in its economic program: while imposing wage restraint and exhorting unions to forego dearly-bought rights, little was done to restrict profiteering or to mitigate the impact of soaring prices. More pragmatic than many of his colleagues, Lloyd George tried to strengthen the hands of the union leaders, impervious to the fact that collaboration with the authorities often undermined their influence with the rank and file. What negotiation failed to secure government officials never hesitated to impose by means of legislation. Only the threat of retaliation by large groups of workers, South Wales miners in 1915 or the engineering trades for example, slowed the pace of state control, but by the end of the war dilution of skilled labor was being extended and guarantees for protected occupations were continuously eroded for the sake of military recruitment.

Wrigley concludes that organized labor, despite the appearance of consultation, enjoyed only the most tenuous partnership with the government. By threatening compulsion Lloyd George forfeited the support of the movement, depriving himself of a potential political base among working class voters after the war.

Admirably researched, this book is marred by irritating flaws. Minor officials are often mentioned without being identified. More careful editing would have eliminated other errors: Sydney Buxton appears as Charles and as Sydney in the same paragraph; Limehouse, scene of Lloyd George's celebrated diatribe against the aristocracy, is transformed at the end of the book, by a process of verbal urban renewal, into Limehurst.

F. M. LEVENTHAL
*Boston University*

ROBERT E. BUNSELMEYER. *The Cost of the War, 1914–1919: British Economic War Aims and the Origins of Reparation.* Hamden, Conn.: Archon Books. 1975. Pp. 249. $12.50.

The author of this compact study of the wartime background of reparations policy shows that British attitudes were directly affected by concern about the effects of the war on British trade and finance and by expectations of economic conflict with Germany after the cessation of hostilities. In part 1 he traces the growth of Anglo-German rivalry through the prewar protectionist and tariff reform campaigns, which left a wartime division in the Conservative party between the free-trade minority, and the moderate and extreme protectionists. He deals with the Paris Economic Resolutions of 1916, proposals for imperial preference, and wartime economic planning for peace—including an interesting memorandum by J. M. Keynes and W. J. Ashley, "On the Effect of an Indemnity." By the end of the war the political appeal of reparation and indemnity had supplanted concern with postwar trade, raw materials, and key industries.

Part 2 covers the intricate negotiations between Britain, the United States, and France, and gives special emphasis to the role of anti-German sentiment during the British general election of 1918. Despite efforts at restraint by leading politicians and Treasury advisers, the demand that Germany should be "made to pay" exerted a major influence on the reparation articles of the Treaty of Versailles.

This is primarily a political study, written in a clear and readable style, but giving comparatively few data on the economic conditions of the period.

A. W. COATS
*University of Nottingham*

ALEX P. SCHMID. *Churchills privater Krieg: Intervention und Konterrevolution im russischen Bürgerkrieg, November 1918–März 1920.* Zurich: Atlantis. 1974. Pp. 389.

Few episodes in Winston Churchill's varied career have been as controversial as his staunch anti-Bolshevik stance during the Russian Civil War. The reputation he earned as a politician who would stop at nothing to crush the fledgling Soviet regime cost him dearly, then and later. To some extent his later failure to rally significant support against the British appeasers of Hitler stemmed from a widespread belief that he was a reckless man who had displayed poor judgment in 1918–1920; consequently, he was not trusted by Liberals or the British labor movement.

Alex P. Schmid's stimulating monograph shows the extent to which Churchill deserves the reputation of an anti-Bolshevik crusader. It is the author's thesis that Allied intervention in Russia owed a great deal to Churchill's drive, to his anti-Bolshevik propensities, and to his ability to convince the members of Lloyd George's coalition

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government of the need to destroy the Bolsheviks. But for Churchill, Schmid contends, Allied intervention would not have assumed the form it did during the crucial year of 1919. The aid given by Churchill and those who shared his views to Russian counter-revolutionaries and non-Russian nationalists helped the Bolsheviks in three ways. They found it much easier to build up a strong army; they were able to exploit Russian nationalism to fight the Poles; and they strengthened their grip over Russian society. The most Schmid is prepared to concede is that Stalin's methods of government cannot be blamed on Churchill. The assertion that Allied aid to the White Armies forced Lenin and Trotsky to divert men and resources that would otherwise have been used to spread their influence in Europe is not discussed at length. Schmid further argues that the politics of compromise followed by Lloyd George and the relief measures associated with Herbert Hoover "saved" Europe from "a social revolution."

Schmid's thesis is based on many different sources. These range from government documents in the Public Record Office in London and the private papers of British statesmen to memoirs and works of other Western scholars. The gaps in the documentation of some of the authorities on British involvement in Russia are noted. British newspapers and *Hansard* have also been combed in search of additional information on Churchill's views and his attempts to convince the British public of the righteousness of his policy. The only significant gap in Schmid's impressive documentation is the Churchill archives, which were not made available to him. These may throw new light on Churchill's sources of information on Russia and on his dealings with Boris Savinkov, the former terrorist who was one of Lenin's most resolute opponents during the Civil War.

IVAN AVAKUMOVIC
*University of British Columbia*

ADELE MASSARDO MAIELLO. *Laburismo e Russia Sovietica, 1917–1924.* (Pubblicazioni dell'Istituto di Studi Storico-Politici, Facoltà di Scienze Politiche, Università di Genova, volume 3.) Milan: Dott. A. Giuffrè. 1974. Pp. iv, 380. L. 6,500.

This monograph deals with the policy of the British Labour Party toward Soviet Russia up to the fall of the first Labour government. Its focus is on the Anglo-Soviet negotiations of 1924 and their collapse. Though less compact and elegant than the comparable study by Stephen Graubard published some twenty years ago (*British Labour and the Russian Revolution*), it supersedes Graubard's volume in its detailed documentation drawn from unpublished records of the Foreign Office, Labour Party archives, and the private papers of Lansbury, MacDonald, Morel, Posonby, Dalton, the Webbs, and others. The present work also probes more deeply into the rich variety of responses to Bolshevism on the part of various components of the Labour Party. Labour failed its "ordeal by fire," says the author, because of a number of factors: the inner tensions in the party which allowed it to proclaim Russia as an equal member of the comity of nations despite its essential hostility to and distrust of the Soviet regime; its traditionalist economic view of Russia as an export market and a supplier of raw materials; the authoritarian tendencies of MacDonald and the erosion of Labour's propaganda energies after assuming office; and excessive reliance on the permanent bureaucracy, particularly the Foreign Office which was hostile to Labour's state policy vis-à-vis Russia. The author sensibly avoids overstressing the Campbell case and the "Zinoviev letter," though, in regard to the latter, she notes that "the behavior of the Foreign Office on this occasion leaves room for many doubts"—not an original position but an exceptionally well-documented one. Though ending abruptly and lacking sufficient overall interpretation, the present work is a useful synthesis of published and unpublished materials on a major, and much discussed, episode in the history of the Labour Party.

RICHARD STITES
*Ohio State University, Lima*

CHRIS COOK. *The Age of Alignment: Electoral Politics in Britain, 1922–1929.* Buffalo: University of Toronto Press. 1975. Pp. x, 367. $17.50.

The central theme of this book is the nature of electoral change at the constituency level between 1922 and 1924, the years in which the Labour Party replaced the Liberals as a governing party in Britain. The subtitle chosen by Chris Cook is therefore a little misleading, since the bulk of the book consists of a dense but closely argued constituency analysis of the three general elections between 1922 and 1924. Cook has made a careful and thorough study of his sources, which include not only private papers, but also party archives and the provincial as well as the national press. For this reason, his book, although it does not make for easy reading, is nevertheless a valuable contribution, not only to the study of electoral behavior during the interwar years, but also to the debate on the decline of the Liberal Party.

Cook modestly argues that his conclusions "do not lend themselves to any startling or dramatic new theory to explain the fate that overtook the [Liberal] party. However, the electoral evidence of

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the period from 1918 to 1929 does emphasise certain factors that have, as yet, not figured fully in the historiography of this subject." The Liberal decline was not evenly spread throughout the country, but was far more rapid in the large cities and mining areas than in rural areas. This leads Cook to the conclusion that the decline of the Liberals came about not, as Trevor Wilson argues, as a result of the war and the split between Asquith and Lloyd George, "but from fundamental and long-term characteristics in the structure, social composition and outlook of the party in the major industrialised and mining areas. This weakness was reflected in the inability of an essentially middle-class party to accommodate working-class candidates, or to formulate a relevant radical industrial policy." These tendencies were already apparent before 1914 and before the advent of universal male suffrage; the extension of the franchise in 1918 served only to accentuate them. Thus the war and the party split, although they may have aggravated the difficulties of the Liberals, did not cause them. As the older sources of political division were disappearing, to be replaced by a division in terms of class feeling, the Liberals ceased to have anything distinctive to offer; and by the time Lloyd George was able to provide the remedy with his Keynesian programme of 1929, it was too late.

Perhaps the next step in this discussion would be to extend it to a comparative context, and to show why class conflict led to a Labour Party in Britain, but not in the United States; what were the peculiar features of the two societies which accounted for the difference? Meanwhile, Cook's study can be warmly welcomed as an important addition to a continuing debate.

VERNON BOGDANOR
*Brasenose College, Oxford*

ELIE KEDOURIE. *In the Anglo-Arab Labyrinth: The McMahon-Husayn Correspondence and its Interpretations, 1914–1939.* (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1976. Pp. xii, 330. $32.50.

The controversies surrounding the negotiation and implementation of Britain's 1915–1916 agreements with the Arabs remain as much alive today as they were decades ago. The endemic Arab-Israeli conflict continues to cast on issues relating to Palestine a continuing political relevance. More generally, ever since the Balfour Declaration promised a national home for the Jews in Palestine and the post-World War I settlement established Britain and France as mandatory powers in political control of large portions of the Arab Near East, Anglo-Arab relations have been embittered by a perception of duplicity and broken promises on the part of "perfidious Albion" shared by all Arab nationalists and many British public officials and scholars alike.

It is this perception that Elie Kedourie sets out to allay by well-researched analysis of wartime archival sources and a lengthy critique of several subsequent Foreign Office studies that sought to elucidate the extent of British commitments tendered to Sharif Husayn of Mecca in exchange for his leading an Arab revolt against the Ottoman suzerain. Kedourie, a noted University of London scholar of Anglo-Arab affairs, is upset at the "corrosive feeling of embarrassment, if not of guilt" that has come to "demoralize British Middle-Eastern policy." He attributes this to British officials' misreading of the evidence in their own files (Harold Nicolson and Arnold Toynbee are among the many taken to task here), their sheer ignorance of some of this evidence, and faulty *ex post facto* exegesis designed to serve political goals of the moment. He endeavors to show that no firm commitments were made to Husayn regarding territorial boundaries of the future independent Arab state(s); that Palestine was definitely excluded from these boundaries; that all British commitments were conditional on uprisings and other anti-Ottoman actions by Husayn and by the Arab movement in Syria that were only partially fulfilled; and finally, that the famous exchange of letters did not constitute a textually self-contained legal contract or binding treaty, but was a series of statements of intention regarding future policy, supplemented by often unrecorded oral understandings and contingent for their proper interpretation on such additional factors as changing circumstances and undertakings to other parties.

Does the author succeed in restoring the good faith of the British government? His exhaustive effort yields mixed results. His reconstruction of the negotiations between the British High Commissioner in Cairo and the Sharif and of internal Foreign Office debates persuasively supports some of his major contentions. On the other hand, he succeeds in establishing beyond doubt that "at its face value, McMahon's language was so qualified that it meant almost nothing, and from his words it was equally easy to infer the exclusion, as well as the inclusion, of any territory whatever." This was McMahon's object. As a plethora of internal communications originating in London, Cairo, Aden, and Jeddah make clear, the British government labored under the constant " 'dread' of disabusing Husayn of his illusions" about the extent of Britain's promises to him lest the Arabs desist from their pro-British efforts (p. 196). Kedourie shows how British officials actually encouraged Husayn to interpret their pledges expansively, dangled before him the alluring prize of the Islamic caliphate,

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and informed him about deals such as the Sykes-Picot agreement partitioning the Levant into British and French spheres of influence, but only in a partial and misleading fashion designed not to alarm him. They further assured him that the Balfour Declaration would be implemented in a manner "compatible with the freedom of the existing [Arab] population, both economic and political."

If Englishmen today feel qualms about the honesty with which their government treated the Arabs sixty years ago, it is not, *malgré* Kedourie, due to ignorance or gratuitous self-recrimination, but because McMahon and company did their job only too well.

PAUL JABBER
*University of California, Los Angeles*

ANN TROTTER. *Britain and East Asia, 1933–1937.* (International Studies.) New York: Cambridge University Press, for the Centre for International Studies, London School of Economics and Political Science. 1975. Pp. xi, 277. $23.50.

The Chinese proverb, "let the water flow and the stones will appear," accurately describes the outpouring of monographs on British Far Eastern policy in the 1930s since the thirty-year rule opened HM's archival sluicegates. Ann Trotter's study joins with those by Thorne, Blickenstorfer, Lee and, of course, Louis, in completing our detailed understanding of this decade in which the lesser-day caretakers of imperial destiny struggled in vain to preserve Greater Britain's empire of extorted privilege and synthetic splendor in East Asia.

Dominating the mid-thirties, Trotter documents convincingly, was a "vigorous," if futile, discussion in Whitehall. The dilemma of the British was cruel: how to safeguard their vulnerable interests in China, handle Japan's slippery demands in Chinese and naval matters, and simultaneously avoid unduly alienating either Nanking or Washington. The Old China Hands at the Foreign Office, quickly concluding that it was impossible to square the circle between Chinese nationalism and Japanese Pan-Asianism, argued that Britain's only safe course was a "wait-and-see" policy (alias: muddling through). Not so the boys at the Treasury. Their exaggerated beliefs in the value of the China market and the persuasiveness of economic bait to Tokyo led Neville Chamberlain (then chancellor of the exchequer) and Sir Warren Fisher (permanent under-secretary) to campaign energetically for an accommodation with Japan. Trotter's thesis is, in fact, that they pushed so hard for an understanding that by 1935 the Treasury was conducting its own foreign policy. The resulting "dual diplomacy"—the Treasury charging on while the Foreign Office philosophized from the sidelines about the dangers of attempting anything constructive—produced only damaging confusion and inconsistencies. With the abject failure of their pet project, the Leith Ross mission, even the grandees at the Treasury had to concede Japan was beyond the pale.

Trotter deserves high marks for her pioneer research in the voluminous British archives (with a titbit or two from Japanese sources). How regrettable, therefore, that her findings are presented in such a leaden way. The signs are all here of a dissertation rushed too soon into print: the convoluted sentences, excessive use of the passive voice, uninspired and in places incongruous footnotes, and, most of all, that steady dissertating tone. Had she shed her dissertation blinders, moreover, and not stopped abruptly with the outbreak of the Sino-Japanese War in 1937, she might have helped her readers see more clearly what this reviewer, for one, regards as a significant contribution: the *déjà vu* overtones of British policy after 1937. Readers of Bradford Lee's excellent *Britain and the Sino-Japanese War, 1937–1939* who came away wondering why Chamberlain and the Foreign Office did so little to appease Japan in the late thirties, why the British were so certain of China's ultimate triumph in the war, and why a snubbed America was so stand-offish will find some intriguing clues in Trotter. Having to read two books, where one would have done nicely, illustrates the questionable wisdom of publishing diplomatic history in two to four year slices.

ROBERT J. GOWEN
*East Carolina University*

ARTHUR MARDER. *Operation "Menace": The Dakar Expedition and the Dudley North Affair.* New York: Oxford University Press. 1976. Pp. xxv, 289. $18.75.

Arthur Marder sets himself three main tasks: to examine Operation "Menace" as a case study in the genesis, execution, and failure of an operational plan; to assess Churchill's role in the enterprise; and to re-examine the controversy over the dismissal of Admiral Dudley North, who, it is often alleged, was sacrificed as a scapegoat for the operation's failure.

On all three counts, the author provides a lucid and expert analysis based upon the available documentary evidence in the Public Record Office and numerous interviews with surviving participants. From the outset, there was a fundamental failure of political intelligence which meant that the entire basis of the operation was misconceived. The British, relying too heavily upon Free French assess-

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ments, thought Dakar would capitulate with only a minimum display of force—a sort of modern-day Jericho with de Gaulle cast as Joshua; even the contingency plans for shore landings were based on this misconception. Strategic intelligence was also poor. The appalling lack of security, so often commented upon, was not in fact of much significance, although it later rebounded upon the Free French. The Vichy government did not realize that the destination was Dakar, and its decision to send the Toulon squadron south was a reaction to events in equatorial Africa, not to dockside talk at Liverpool. There were many avoidable delays, and communications were poor. In short, Operation "Menace" was aptly named only in reference to itself. Churchill bore a heavy responsibility, and most of the problems arose from the great haste he injected into the enterprise from the beginning. The Chiefs of Staff were never keen on the enterprise. The force commanders emerge with some credit, as, indeed, does de Gaulle, who maintained his dignity throughout.

If Churchill's responsibility for the operation was major, he is absolved by the author of the charge of having cast Admiral North as its scapegoat. The Admiralty, primarily in the shape of its First Sea Lord Dudley Pound, had lost confidence in North before he permitted the French squadron to sail unmolested through the Straits of Gibraltar. This incident was merely a catalyst, and, the author implies, the decision to query North's conduct was made before the failure of "Menace" became apparent. This is the least satisfactory section of Marder's book. By his own admission (p. 225, note 44) he bases his absolution of Churchill only on the absence of written proof; in the light of contrary verbal recollections, he has given the benefit of doubt to Churchill. This is not altogether convincing, especially in light of Churchill's postwar treatment of North. Nonetheless, Marder, while inclined to be less "pro-North" than Roskill is in his offical history, convincingly demonstrates that North was shabbily dealt with by the Admiralty up to his partial rehabilitation by Macmillan in 1957. His discussion of the opposing arguments and his weighing of the evidence is judicious and skillful and will probably be the last word on the matter.

DAVID STAFFORD
*University of Victoria*

JANET P. MORGAN. *The House of Lords and the Labour Government, 1964–1970*. New York: Oxford University Press. 1975. Pp. xii, 254. $25.75.

This well-informed analysis of the Lords, at once technical and sophisticated, is most welcome as an up-dating of P. A. Bromhead's *The House of Lords and Contemporary Politics, 1911–1957* (1958). Janet P. Morgan's account of "the ambiguous role" of the Lords "in the whole political arena" extends somewhat beyond the limits in her title in both directions. But most significant is her inquiry into parliamentary procedure and behavior, contrasting the Lords and the Commons, in the crisis of 1968–69.

This study will have different uses for different people. Students of constitutional history, and, one hopes, general readers as well, will find their knowledge of parliamentary history widened and their understanding sharpened. The student of government will be at once drawn to the minute dissection of the Lords, its membership and business, supplemented by detailed tables in the appendix. And the historian of Labour will follow closely the politics of the period. But all will become absorbed in the examination of the first major effort to reform the Lords' membership, this in a context of a Conservative-dominated Lords and a Labour-controlled Commons.

It is a complex story, with the important components narrated separately, and thus to be read with close attention or not at all. Morgan covers the promising Inter-party Conference created in November 1967 to implement the Queen's Speech—"to reduce the present powers of the House of Lords and to eliminate its present hereditary basis"; the breakdown of the Conference with the defeat in the Lords of the Southern Rhodesia (United Nations Sanctions) Order; Labour's own Parliament Bill (introduced December 1968) with its two tiers of membership (voting and non-voting); and the end of this bill in fiasco largely through Labour's own tactical blunders. The many paradoxes are effectively brought out by the author. The Lords seem to emerge with the honors, more independence of judgment, more caution and restraint; indeed their conduct in this episode shows why they have survived. Some matters are controversial, and no doubt Morgan will be challenged at certain points. But it is likely that her study will stand its ground quite well.

ALFRED F. HAVIGHURST
*Amherst College*

WALTER ROLAND FOSTER. *The Church before the Covenants: The Church of Scotland, 1596–1638*. Edinburgh: Scottish Academic Press. 1975. Pp. viii, 216. £5.00.

Scotland's Reformation success of 1560 did not suddenly establish the pattern of Presbyterianism which would ultimately triumph. Although the Reformed doctrines were adopted and generally followed, the episcopal tradition lingered. Not until the 1580s were the bishops' powers substantially

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curbed. In order to enhance royal power, James VI strengthened the bishops' authority during the period from 1596 to 1610. The presbytery versus episcopal controversy was sharpened as a result of royal enlargement of bishops' powers; nevertheless, the kirk sessions, presbyteries, synods, and general assemblies continued to flourish as effective instruments of church government even during the era of episcopal revival.

James VI persuaded parliament to restore episcopal temporalities in 1608 and prompted the Glasgow Assembly of 1610 to establish two courts of high commission, which gave the bishops, and especially the two archibishops of St. Andrews and Glasgow, increased status and authority. The crown began to give its presentations to bishops instead of to the presbyteries, and increasingly the bishops had to approve church policies and decisions before they were final. James also restored the practice of bishops sitting in parliament.

Progress was made during this period toward developing an educated clergy. The author's research reveals that by 1638 a university education was standard for ministers, and it was rare to find a minister who was not a university graduate.

During this era, bishops presided over presbyteries or appointed moderators over them, held the final power over excommunication, disciplined ministers, and made visitations in their dioceses. Many ministers opposed the strengthening of bishops, and some ministers were ousted because of their opposition to episcopacy. Despite the episcopal revival, the kirk sessions, presbyteries, and synods remained strong. For a few decades this two-track system of church government operated in Scotland. Calvinist doctrine and practices, however, were otherwise unaltered.

Walter Roland Foster has successfully portrayed the structures of church government in this scholarly study. He has also treated the related topics of ministers, finance, and church discipline after extensive research in presbytery and synod records and many other extant contemporary sources. Except for an occasional repetition, the book is well-written, and it is a valuable addition to the literature on the early period of Scottish Protestantism.

RICHARD GLEN EAVES
*Auburn University*

S. G. CHECKLAND. *Scottish Banking: A History, 1695–1973*. Glasgow: Collins Publishers. 1975. Pp. xxvi, 785. £8.25.

This book provides a detailed, comprehensive, and scholarly account of the development of Scottish banking institutions over the past two and three-quarter centuries. The emphasis is on the period before 1850 when the system exemplified the innovativeness and flexibility which can be produced within a competitive economic environment. Banking in Scotland (unlike England) operated without substantive governmental intervention, limitations on capital size, or a "public" banknote-issuing monopoly, and with "small" notes. Amid frequent economic disturbances, the Scottish banks pioneered a sequence of institutions tailored to rapidly emerging organizational, geographical, and financial needs. These included banking under limited liability, joint-stock financing, branch systems, cash credit systems, and a reliance on notes and deposits and a minimum of specie. The system's competitiveness was largely lost with the Act of 1845 which denied the right of note issue to new banks. The consolidation of the system and its decline (in favor of the English banks) as a major force in world banking followed. The book describes with great clarity the complex legal, political, sociological, and economic underpinnings of these and related occurrences.

The analytical aspects of the book are unfortunately less successful than the excellent historical description. For example, the judgment that the Scottish system was "highly volatile" is dependent upon implicit comparisons with modern bank-failure experience at the expense of the potentially more interesting contemporaneous English comparison. Another example is the assumption that a simple trade-off existed between the tendency toward "conservative" lending practices of many banks and the system's contribution to Scottish economic development. Instead, "conservatism" also facilitated development by increasing confidence in banks and bank money. Finally, the fundamental question of the relative importance of post-1845 noncompetitiveness and the growing economic dominance of the English economy in the relative decline of Scottish banking is barely addressed.

In spite of these difficulties, S. G. Checkland makes an important contribution to the literature of comparative banking systems and institutional change by doing an exceptional job of presenting the facts of a major case study. An additonal contribution is the construction of valuable data in rare detail on Scottish bank operations since 1700. Thus the book is capable of providing the basis for much additional research on this important topic.

JOHN P. JUDD
*Federal Reserve Bank of New York*

A. L. DONOVAN. *Philosophical Chemistry in the Scottish Enlightenment: The Doctrines and Discoveries of William Cullen and Joseph Black*. Edinburgh: Edinburgh University Press; distributed by Biblio Distribution Center, Totowa, N.J. 1976. Pp. x, 343. $17.50.

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The recent historiography of eighteenth-century chemistry, particularly British chemistry, affords a striking example of the transformation of the history of science into a full-fledged discipline. The studies of Robert Schofield and Arnold Thackray have placed the development of chemistry in the mainstream of eighteenth-century ideas and culture. These authors, and others such as A. E. Musson and Eric Robinson, have also provided the context for eighteenth-century chemistry in the needs of a commerical and incipient industrial society, although the precise interaction between the chemists and the technologists remains a matter of controversy.

A. L. Donovan's study of the Scottish chemist William Cullen and his illustrious pupil, Joseph Black, is in the vein of these recent works. He has written what he calls a "joint intellectual biography" of these two scientists and teachers, and has tried to set their chemical thought and investigations in the complex intellectual setting of the Scottish Enlightenment, in which the ideas of Newton and Locke were given new orientation and interpretation by the Scottish empiricist philosophers such as Francis Hutcheson and David Hume. Cullen and Black's careers are placed in the social and cultural context of the swiftly modernizing Scottish universities, which became the pre-eminent intellectual and scientific centers of the eighteenth century.

Roughly three-fifths of the book are devoted to Cullen and the last two-fifths to Black. Neither receives full biographical treatment. Instead, Cullen's career and the development of his chemical thought are studied for the period down to the 1750s when he was Black's teacher and mentor, and Black's career and scientific work down to the 1760s, to the completion of his major scientific investigations. The *raison d'être* of the book is the study of Black's great scientific achievements: the investigation of causticity and fixed air in *magnesia alba* and the research on heat phenomena. The bulk of the section on Black is a detailed analysis of this chemical and calorimetric work.

The emphasis of Black's scientific achievements provides good thematic unity, but it also results in a narrowing of the book's character. Donovan's treatment of Cullen is a much richer study of intellectual and social history than is his treatment of Black. The historian of science will, however, find the extended discussion and analysis of Black's scientific work convenient and useful.

SEYMOUR MAUSKOPF
*Duke University*

EMMET LARKIN. *The Roman Catholic Church and the Creation of the Modern Irish State, 1878–1886.* (Memoirs of the American Philosophical Society, number 108.) Philadelphia: the Society. 1975. Pp. xxiv, 412. $7.50.

For anyone who is not a specialist in modern Irish history this publication will almost certainly be puzzling, and many general readers may find it impossible to read with profit. It really is not a book, at least not in the conventional sense of being a self-contained and self-sustaining narrative or argument. But to say this is not in any way to condemn the publication. For nearly two decades Emmet Larkin has been engaged in a multi-volume history of the Roman Catholic Church in Ireland from the late penal times to the Irish revolution. Such a long-term project is as courageous in its undertaking as it is imaginative in its conception. This is a first fascicle of that larger project, although Larkin modestly does not mention this.

The volume, therefore, rests on invisible foundations which, presumably, will be published at a later date. The story begins *in medias res*, hurtling the reader into a massively detailed discussion of the secular politics of ranking Irish churchmen. The structure of the Catholic Church in Ireland is not discussed, and its theology and pastoral activities are ignored. After reading this volume one has to remind oneself that there were indeed clergymen below the rank of bishop, and that the Church had a religious importance as well as a secular impact.

Larkin's thesis is two-fold: first, that the Church achieved a constitutional relationship within the Irish state which "prevented that state from being eventually turned into the worst kind of autocracy by either the leader or the party," and second, that it was in the years 1878–86 that the Irish state was created.

Whether or not one is convinced by Larkin's arguments, one has to admire his diligent research and, especially, the skill with which he tells episcopal anecdotes. Even given the economic exigencies of present-day scholarly publishing, it seems an injustice to Larkin's industry that he was not able to provide full footnotes to the correspondence cited or to present more than a one-page bibliographical note.

As to the work's two theses, this volume does not conclusively support them, but one fully expects that the additional material in forthcoming volumes will do so. In order adequately to make his point, Larkin will need to do several things: first, in regard to his statements about postrevolutionary Ireland, he must establish that the worst kind of autocracy by either leader or party was a reasonable possibility; second, that it indeed did not occur; and third, that it was the Church which prevented the rise of autocracy.

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More important, to establish his second argument, Larkin must deal directly with the brilliant recent work of David Miller, *Church, State and Nation in Ireland, 1898–1921*. Therein Miller presents and convincingly documents a thesis akin to Larkin's. But there are important differences: whereas Miller argues that Church-State relations in post-revolutionary Ireland can be understood partly in terms of the prerevolutionary framework, Larkin argues that the nature of the state itself was determined by prerevolutionary conditions. Also, Miller emphasizes that the decades immediately before the Irish revolution were the crucial ones in determining later events, while Larkin opts for the earlier, Parnellite period. In my judgment, Miller's work at present is the more convincing, but one awaits Larkin's further volumes.

Finally, to validate his two themes, Larkin must prove that his equation of the Roman Catholic Church in Ireland with the Roman Catholic bishops is valid, and that his simultaneous equation of the Parnellite political apparatus with the Irish state also is justified. My own belief is that in the future he will deal satisfactorily with these matters and that this present publication will be seen as one facet of a magnificent historical achievement.

DONALD H. AKENSON
*Queen's University, Kingston*

B. L. REID. *The Lives of Roger Casement*. New Haven: Yale University Press. 1976. Pp. xix, 532. $25.00.

B. L. Reid re-examines the story of the Protestant Irishman who abandoned a distinguished career in the British Consular Service to become a self-appointed Irish nationalist envoy to Germany during World War I. Though knighted by the British in 1911 for revealing racist atrocities in the Belgian Congo and the Amazon, Roger Casement was hanged by his government five years later for his role in the abortive Easter Rising in Ireland.

Casement's incongruous behavior, according to Reid, resulted from his life-long identity crisis, which reflected the tensions within Anglo-Irish society—tensions that fragmented his personality "to the point of disastrous incoherence." The so-called "forged diaries" which revealed Casement's secret homosexuality, and which the British furtively circulated to discredit him, are affirmed as authentic on the basis of all available evidence. But Reid finds them "tiresome as a fact and as a problem." He asks acceptance of the man as he was: neither a saint, as some Irish think, nor a sinful villain, as many British say.

Both Irish and British scholars should hail Reid's provocative analysis, which captures the elusive personality that escaped Brian Inglis in *Roger Casement* (1973). Reid's Casement is a "divided spirit pursuing wholeness," whose schizoid nature reflects the present conflict in Ireland, and in a larger sense "expresses a condition even more general in the culture of us all." Some readers may now admire the man less, but they can at last begin to understand him. Reid may not have laid Casement's ghost to rest—some relevant documents still await official release—but he has come closer to accomplishing that difficult task than has anyone else. He castigates the British, not for sending Casement to the gallows as a traitor, but for the way in which they cut down and destroyed a powerless man with weapons that had "no relationship to the crime that had put him in their power."

This masterly biography enhances the quality of the literature devoted to one of Ireland's most enigmatic figures, and to this particular phase of Anglo-Irish relations. It adds luster to Reid's earlier reputation as a Pulitzer Prize-winning scholar.

VIRGINIA E. GLANDON
*University of Kansas*

HENRY SUMMERFIELD. *That Myriad-Minded Man: A Biography of George William Russell, "A.E.", 1867–1935*. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. xiii, 354. $18.50.

George Russell was indeed myriad-minded, and to talk fully about his work requires statements about painting, poetry, religion, mysticism, journalism, agricultural cooperation, and Irish history in the twentieth century. Before becoming assistant to Horace Plunkett in the Irish Agricultural Organization Society in 1897, Russell had been a clerk and cashier at Pim's, a large Dublin drapery establishment. But his real life had been in the Theosophical movement. Henry Summerfield, in three fine opening chapters, discusses Russell's involvement with theosophy, his quest for spiritual perfection, and his developing talents in painting and poetry. His work with the Irish cooperative movement was to reveal practical talents of a rare kind, and for eight years Russell traveled widely in rural Ireland, coming to understand, as few intellectuals did, the nuances of Irish life.

In 1905 he settled down in Dublin as editor of *The Irish Homestead*, the weekly journal which had been originally founded to support the Irish Agricultural Organization Society. The periodical gave Russell a world audience, and his articles caught the attention of readers whose interests lay far beyond professional agriculture. The journal's headquarters at Merrion Square, and Russell's home in Rathgar where he and his wife welcomed visitors on Sunday evenings became, in the years before 1914, centers of Dublin intellectual life.

For his contemporaries, Russell's greatest dis-

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tinction lay in his kindness, his democratic spirit, and his affirmative attitudes to life. He frequently spoke against injustices of every kind, and on October 7, 1913 sent an open letter to the *Irish Times* on the Dublin strike, calling the masters of industry bad citizens, bad employers, and bad businessmen. A pacifist, Russell could not sanction either the World War or the Irish Rising, but in his famous poem, *To the Memory of Some I Knew Who Are Dead and Who Loved Ireland*, he honored those who fought on the Western Front and those who, however mistakenly in his eyes, died as a result of the 1916 rising. He participated in, but later resigned from the Irish Convention. In 1921 he urged the acceptance of the Treaty, arguing that to reject it was to destroy the nation. Earlier, Russell had expressed the view that partition would destroy a desirable balance between the industrial North and the agricultural South, and would not assuage, but rather keep hatred alive. He was mindful, however, that Ulster might require some kind of autonomy, perhaps on the Swiss or Canadian pattern. In 1923 *The Irish Homestead* was incorporated within *The Irish Statesman*, and as editor of the new publication, Russell became, in the words of the author, the conscience of a nation.

Summerfield's book is based on extensive research, is interestingly written, and evocative of atmosphere. It is the only full study since John Eglinton's brief *Memoir* of 1937. Russell appears as a very public man, but one would like to know more about his family life, his wife and children. Mrs. Russell had once, as Violet North, edited the *Irish Theosophist*, but in this work she is a shadowy figure. Also, it would be interesting to examine Russell's editorial work within the broader tradition of Irish journalism. Perhaps we shall hear something of this in Summerfield's forthcoming volume of selections from Russell's articles and editorials.

HELEN MULVEY
*Connecticut College*

GEORGES DUBY and ARMAND WALLON, editors. *Histoire de la France rurale*. Volume 1, *La formation des campagnes françaises des origines au XIV^e siècle*; volume 2, *L'âge classique des paysans, 1340–1789*; volume 3, *Apogée et crise de la civilisation paysanne, 1789–1914*. (L'Univers historique.) Paris: Seuil, 1975. Pp. 620; 620; 568.

Written in cities, or by urban-oriented chroniclers, history has generally been about cities, their doings, and their values. Most human beings (ninety-five percent or so through the Middle Ages, eighty percent at least around 1789) lived rural lives. Elites, however, including warriors and priests, were urban-focused and have remained so—especially in France. There, the first serious attempt to integrate rural history into history "proper" came only in our lifetimes: Marc Bloch's *Caractères originaux de l'histoire rurale française* (1931). Despite a vigorous record since, the scholars involved and the pages turned out can scarcely compare (at least in numbers) with those devoted to bourgeoisies or urban workers.

A history of rural France from origins to present is therefore particularly welcome, not only as an introduction to a still neglected field, but as a summing up of work to date, a running indication of work still to be done, and a convincing demonstration of what can be achieved in plumbing realms long considered beyond the historian's ken. Never before has such a diverse mass of data (and of inferences) about popular cultures, mentalities, conditions, practices—and those of the illiterate, most popular of all and most difficult to fathom—been made available in one place. Each volume is endowed with a thorough index—a rare delight for students of things French, though become less rare of late. Each carries a brief, selective, but excellent bibliography, including works published as late as 1975 (crucial in a field where scholarship is moving fast). Illustrations are numerous, fascinating, and worth the price of admission for their sake alone. Maps, also numerous, are useful and suggestive, though marred by a design that makes them difficult to read. All in all, to begin with the conclusion, this is a splendid and welcome work.

A critical review, even the most cursory account, of the material contained in 1,737 pages of text (a fourth volume will provide another 600 pages) is more difficult to produce. The first volume is dominated by the contributions of Georges and Claude Bertrand (the ecological background) and Guy Fourquin (from the dark fifth century to the doomed heights of the thirteenth). The Bertrands lay down some basic themes: What we describe as "nature" is an ongoing creation, largely at the peasant's hand. The earth itself, at least the accessible part of its crust, constantly modified by the biological effects of seeds and roots, micro-organisms—or moles, is a living organism in constant evolution. As far back as we can see, animal and vegetal life are not "wild," but in complex relationship with human enterprises and timetables. Rural life in turn exists as a function of this symbiosis: the agricultural calendar is the wisdom of weather(s), of knowing how to use the seasons and their changes. Climate dominates the rhythm of country living, the round of labor and feasting, the calamities that threaten crops and life. The "*ordre éternel des champs*" is a nostalgic fancy of nineteenth-century city dwellers: reality consists of routines (of which catastrophe is a part), of mul-

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tiple constraints and repeated adjustments to evolving necessities. Upon this, history, which is about perceptible change, superimposes its accidents and its trends.

By the thirteenth century clearing and drainage had established the basic features of the French countryside. Population growth (in the eleventh century, already, the land was more densely settled than any of its neighbors) had pushed back forest and brush, water and marsh, waste and wilderness, more than at any other time since prehistory. Pressure of rural populations provided one impetus to vigorous agricultural expansion, motor of economic take-off in textile production, and other artisanal developments. In the early fourteenth century the expansion peters out. Plague, war, and taxes ravage the countryside (1340–1450); let up to permit a modest resurgence (1450–1550) when new dietary resources—chestnut, olive, buckwheat—also appear or spread; then return in worse guise for another disastrous century and a half.

Volume 2 (dominated by the chapters of Le Roy Ladurie) demonstrates the role of climate: the little ice age that begins around 1580 and culminates in 1710 or so counts more than the wars, foreign and domestic, of those morose years. It affected more than weather; Reformation and Counter Reformation made for an austerity well suited to dearth. But wars also counted—in ever heavier taxes, in the immediate depredations of the men-at-arms. For peasants, more than for the burghers with their stores of grain and their city walls, war was suffering, destructions, exactions, waste. Production and exchanges were disorganized, living conditions catastrophically affected. In this context, what matter the ideologies and political divisions of urban or aristocratic parties? Austere in diet as they were in sex (by necessity in both cases), the peasants paid for the irrelevant abstractions of alien authorities. The most they could do (and not all managed) was to survive. Rebellions, risings, revolts, "*émotions*," were few—surprisingly few when one considers the crushing taxes, the constant bleeding, which perhaps help to explain their paucity.

But the ice begins to melt in the warmth of Le Roy Ladurie's lively pen, and half of volume 2 is dedicated to the relatively happy eighteenth century when affection (or playfulness) and contraception begin to tread increasingly prosperous glebes bulging with survivors. The high birth rate, tempered by calamities and by homicide (the latter at three times the rate of 1971 Harlem!), falls dramatically. Rural delinquency also is "civilized," takes on urban aspects, and violence grows less. Crime shifts from person to property, from massiveness to marginality. Better communications allow a larger population to be fed, eliminate famines, and help provide the 1800 calories a day that make the average pittance of 1789. The *cabaret* becomes a focus of new consumptions (wine, tobacco), sociabilities, and subversions. Grumbling resignation gives way to rising expectations, city bred. Dumb insolence grows less dumb. Literacy nibbles at the dark—less under the impulse of Enlightenment ("*Je vous remercie de proscrire l'étude chez les laboureurs,*" writes Voltaire to La Chalotais; "*Le pauvre n'a pas besoin d'éducation,*" declares Rousseau) than under that of the Counter-Reformation Church.

Even then, as Jean Jacquart remarks of the seventeenth century, peasant civilization remained a civilization of the spoken word. One may add, of words seldom spoken in French. Local speech, bilingualism, and "sabirization," going back to the Roman colonization of Gaul, remained the rule in the countryside. As for the reforming Church, I doubt whether the faith of country people became "deeper and purer" as a result of its seventeenth-century labors. Rural observance may have been better structured. But familiar rites had been upset; and rites were what their "religion" was about. To the extent that "superstitions" were suppressed (they were not really until the nineteenth to twentieth centuries), the Church only stood to lose, not gain.

Le Roy Ladurie is a hard act to follow, but volume 3 keeps up the pace. It too is a mine of information: demography, productivity, prices, wages, profits from land, from labor, from the market, taxes, the price or rent of land, the legislation that affects it and its people, social (and economic) mutations, divisions, stresses, strains, frictions, and reorientations. Above all, though, it describes the peasant's passage into the modern world, admirably treated by Gabriel Désert and, especially, by Maurice Agulhon.

Crucial though the Great Revolution is in this respect, at the rural level national politics remain an alien, often unsuspected, irrelevancy. For Agulhon, 1830 opens the way to change or at least to evolution toward political change. Qualified though his view is, it sets the date too early for country people, apart from *vignerons*. The local party struggles to be found in many a commune from 1830 (and even after 1848) were little more than Montagu-Capulet feuds prolonged or reenacted under new flags. True, those participating in them were getting used to the new flags and thus to the new implications and possibilities new terminologies portended, but the adjustment took time. Again it may be, as Agulhon would have it, that the apprenticeship of rural democracy began with the elections (especially the municipal elections) of 1848. But the apprenticeship would be long from

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archaic localism to modern, national perceptions. True, the 1830s had staked great landmarks: 1831, election of municipal councils; 1834, public instruction at the village level. But it would be roads and rails, especially the local ones built after 1880, that linked the isolated farmer to cities, nation, markets, hitherto irrelevant or unperceived, that integrated him into a national economy, eventually a national politics, at last becoming meaningful and real. A tardy revolution, but a crucial one, completed only in the twentieth century.

Agulhon and G. Désert bring out the importance of the years 1880–1914—"the great commotion" —as they entitle this watershed between traditional and modern times. This is when new communications permit demographic decompression and irrigation of desolate lands. Limited possibilities expand: lime, fertilizer, exchanges, production (though less than had been thought); diet improves; there is more money about, there are more goods, more ideas too. Traditional folklore dies, to be replaced by the stuffed "folklore" of museums and nostalgia. The "savage" becomes a Frenchman. And Agriculture is recognized as deserving a ministry of its own (1881). The political question remains debatable, since new electoral bottles can contain (and mask) the old wine of traditional divisions and aspirations. At any rate, a new political folklore takes root. And a sense of being part of the nation.

Continuity, renovation, or sea-change? "You pays your money, and you takes your choice." Whatever the choice, the money will have been well spent. Rural (like urban) France remains a jigsaw of diversities. This vast enterprise has surveyed and etched it in stark, vivid shades: *De omni re scibili, et quibusdam aliis*!

EUGEN WEBER
*University of California, Los Angeles*

JEAN THIRY. *Les années de jeunesse de Napoléon Bonaparte, 1769–1796*. Paris: Éditions Berger-Levrault. 1975. Pp. 298.

On the broad and muddy flood waters of Napoleonic historiography much floats, even from time to time a stout vessel or occasionally a noble flotilla. Thiers' twenty-one volumes are still useful; Madelin's sixteen have become standard; Thiry's twenty-eight have attracted less notice. This is in part because they have been written as the spirit moved him, rather than in any chonological or thematic way. Thus the present volume is the twenty-seventh to appear of what has now become the *Collection Napoléon Bonaparte*, the author's first publication on a Napoleonic subject having appeared as long ago as 1922. This set differs from its predecessors not only in its composition and coverage, and in being more up to date, but in some important respects that make it an outstanding reference tool. Each volume has *real* footnotes, an index, and a copious bibliography and list of sources. In some volumes archival documents and books are noted with call numbers. Thiry, unlike many other writers, has consulted documentation outside France. Of the volumes published several have been awarded prizes, and the series as a whole has received another award. The undertaking is clearly significant and deserves to be acquired by more libraries.

The present volume is written clearly, avoids entanglements in the accretions of legend, and skirts the swirls of controversy. The book, however, understandably shows signs of haste. The bibliography lists nothing after 1968, though there were important materials in *Problèmes d'histoire de la Corse* (1971), and Pierre Lamotte has published more articles than are listed here. The titles of books in the notes are not always identical with those in the bibliography, which does not include all the works that appear in the notes (for example, Antommarchi's *Memoirs*). Thiry quotes Masseria's "*Mémoires*," but, as he reproduces in his notes an error previously made by Masson, it is clear that he knows Masseria only second-hand. This is made obvious by the bibliography, which lists a work with a common title in two places, with two dates, and by two separate authors. In fact the work by Masseria is part of the rare book by Peter Moore. Masseria is listed once in the index but at least five times in the text. Matteo Pozzo di Borgo is omitted from the index. Thiry quotes C. A. Pozzo di Borgo's contemporary testimony, citing a reference to Ordioni's book by Madelin, but did not himself look up either Ordioni or the more documented Maggiolo, though his bibliography includes the highly derivative Albertini and Marinetti on the same subject. The Corsican archives contain more documents than Thiry lists (without call numbers), as a recent special issue of the *Annales historiques de la Révolution française* (1974) makes plain. We have a reference to Admiral Thugut (for Truguet), the tricolor flag in the 1770s, and a note about the single Corsican officer in French service when Napoleon got his commission, though there are references elsewhere to Buttafoco in profusion. Napoleon's remarks, while on Saint-Helena, about his youth, cited here are drawn mainly from the dubious Antommarchi, ignoring for example Bertrand's *Cahiers*.

In short, this individual work should not receive any prizes and falls short of the high standards of the other volumes. It needs revision, does not supersede other works, and must be used with caution. Taken all in all, however, we are not likely to

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see again anything like the complete *Collection Napoléon Bonaparte*, and it is indeed to be hoped that Thiry will manage to complete the final volume of a great historical undertaking.

J. M. P. MCERLEAN
*York University, Downsview*

JONATHAN R. DULL. *The French Navy and American Independence: A Study of Arms and Diplomacy, 1774–1787*. Princeton: Princeton University Press. 1976. Pp. xv, 437. $20.00.

Despite the title, this is actually a study of the impact of naval affairs on French policy during the American Revolution. The basic French objective in that war, Jonathan R. Dull holds, was to destroy Britain's monopoly of American trade in order to cripple her domination of maritime commerce. The tragedy of France, he concludes, was that the adoption of this policy eliminated any possibility of social and economic reforms which might have regenerated the French monarchy. To this traditional explanation Dull contributes an ingenious and well-put, if not necessarily convincing, exposition on the importance of naval policy in the formation of French strategy.

In no sense an operational story, the book moves at ministerial level. It describes the great fiscal and logistic difficulties with which eighteenth-century administrators had to contend, and it chronicles with new detail the problems of the Franco-Spanish alliance. Dull, as might be expected from a diplomatic historian, is much more at home describing the latter.

Dull has worked exhaustively in the French Archives. He has used records seldom visited by scholars and largely virgin insofar as American students are concerned. Since he has consciously limited his canvas to the view from Paris, it is not surprising that he has failed to note some highly relevant American works like Charles Lee Lewis's *Admiral De Grasse and American Independence*.

Dull is a young scholar of promise. He researches deeply, if narrowly, but his writing on occasion betrays brashness and a lack of understanding. Princeton University Press deserves recognition for a well-edited, designed, and laid-out book which sports fewer typos and fluffs than are common today. And they put the footnotes at the bottom of the page!

K. JACK BAUER
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JOHN G. GALLAHER. *The Iron Marshal: A Biography of Louis N. Davout*. Carbondale: Southern Illinois University Press. 1976. Pp. xi, 420. $15.00.

John G. Gallaher has filled a gap in the literature of the Napoleonic period with *The Iron Marshal*, his biography of Marshal Davout. He has succeeded in presenting an unbiased view of Davout in this first biography of the marshal in English, whereas the French biographies tend to be partisan in nature, either derogatory or apologetic. He has placed the marshal clearly in the context of the events in which he was a participant. The role of Davout is fully presented, neither overemphasized nor understated. His character and personality emerge from the narrative, and he comes alive as a person. While the portrait is highly favorable, the faults of Davout are not glossed over.

Davout was not a dramatic figure like many of Napoleon's associates. He was solid, thorough, and dependable, a stern taskmaster and a strict disciplinarian. His exacting standards of performance alienated not a few of those with whom he worked, but they made his Third Corps into the finest body of troops in the First Grand Army with the possible exception of the Guard. Davout and his troops repeatedly bore the brunt of the action in the imperial campaigns and almost invariably with success. One needs only to cite Austerlitz, Auerstadt, Eylau, Eckmuhl, Wagram, Borodino, and Hamburg. Only during his stint in command of the rear guard on the retreat from Moscow was his performance not all that could have been expected.

Davout's service as imperial administrator in Poland and Germany rivaled his military contributions to Napoleon and did much to maintain the empire. He loyally carried out Napoleon's policies, no matter how harsh or unpopular they might be. Loyalty was, in fact, an outstanding quality of his character. He invariably accepted whatever assignment Napoleon gave him, even if it were contrary to his own preferences, as when he served as war minister rather than in the field during the Hundred Days.

Gallaher has given us a sound biography of one of Napoleon's most faithful servants and ablest generals. The book reads well and is a valuable addition to the literature of the Napoleonic Period. We hope the same will be done for other prominent marshals.

ROBERT S. QUIMBY
*Michigan State University*

ROGER L. WILLIAMS. *Manners and Murders in the World of Louis Napoleon*. Seattle: University of Washington Press. 1976. Pp. xiii, 206. $9.95.

The effort of professional historians to appeal to general readers as well as specialists is commendable. Roger L. Williams claims to have addressed himself to a nonprofessional audience in

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his *Manners and Murders*, while at the same time warning experts that his latest work is a serious exercise in social history. Williams's previous studies of the Second Empire compel historians to examine this injunction in earnest. His intent is to provide—through a description of seven critical cases, five of them murders—a social history of judicial procedure and to demonstrate how in nineteenth-century France "the court of law became the safest arena from which to express opposition to the established regime." Unfortunately, this study labors uncomfortably between vignette and dissection, and although it is enlightening in many respects, *Manners and Murders* cannot fully satisfy either the professional or the general reader.

The style is appealing, although it froths with such unfortunate ellipticisms as to "do a Lamartine." The general audience is likely to be confused by arcane references to the nineteenth-century background and judicial nomenclature. Historians familiar with the period will be alarmed that Williams relies heavily on a single source, the unofficial *Gazette des Tribunaux*, which although rich in detail is not a stenographic record, and otherwise on memoirs and popular works. Williams's reconstruction of these cases retells familiar anecdotes but also succeeds in making vital points. In discussing the trial of Louis Napoleon after the descent on Boulogne, he explodes the role of Miss Howard. The Orsini case provides him a welcome opportunity to exhume the "good Napoleon III," and the libel action of Trochu against *Le Figaro* a chance to rehabilitate the general and decry the behavior of Eugénie. The most fascinating chapter examines three melodramatic murders, including the notorious Victor Noir case, and shows how opposition elements managed to profit by exploiting public outrage and panic. The analysis justifies Williams's argument that the format of French criminal trials provided unrivaled opportunities to attack entrenched authority. For historians of modern France this lesson is hardly novel; *Manners and Murders* supplies new detail about the social ramifications of sensational judicial proceedings, but the conclusion Williams draws is as old as the Dreyfus Affair.

BENJAMIN F. MARTIN, JR.
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PAUL PRESTON, editor. *Spain in Crisis: The Evolution and Decline of the Franco Regime*. New York: Barnes and Noble. 1976. Pp. 341. $15.00.

The original impetus for the publication of this book was a conference held at the University of Reading in December 1974, almost exactly midway between the assassination of Prime Minister Carrero Blanco and the death of Franco. With one minor exception the demise of the dictator is not discussed, but this book may still be regarded as treating the whole of the Franco regime, for by 1974 the personal rule was coming to an end. The ten articles comprising this volume are divided into two parts: "The Regime," which deals with the power structure, and "The People," a tendentious label for the opposition. All are of high quality, are well researched and clearly expressed.

In the introduction, Paul Preston identifies the crisis referred to in the title as the demand for democratization arising even from those groups that had traditionally supported Francoism. An organization that remained immune and hostile to this current is the Falange, which is the subject of Southworth's contribution. His definition of fascism and its circumstances is procrustean and debatable, but the overall analysis is masterly and insightful. Whereas the Falange remained adamant, the other traditional pillars, the army and the Church, became politically divided. Garcia discusses developments in the armed forces that resulted in either depolitization or political heterogeneity, and Cooper examines the deeper and more influential changes in the Church, which he terms in a subtitle as the development "from crusade to Christianity." Estéban's treatment of economic policy has serious flaws. While the anti-Franco stance of the other contributors is apparent, in Estéban's article prejudice is allowed to mar analysis and judgment. Moreover, he pays little attention to agricultural policies.

The editor's own essay on the opposition, curiously misplaced between chapters on economic groups, is admirable particularly in its lucid discussion of a complicated subject. Sevilla-Guzmán's article on the peasantry and Ellwood's on the working class show the constant disaffection of both groups and their discontent, as manifested in mass exodus from the land and greater labor militancy. The universities, one of the vanguards of opposition, are discussed by Giner in an article that is virtually a reprint of an older one by him—plus a few new pages added for updating. The last two articles are on regional opposition. Unfortunately, Hollyman deals specifically with E.T.A., thus reinforcing the misconception that identifies Basque nationalism with a minority organization composed of a few hundred members. Jones's discussion of Catalanism is wider in extent and more illuminating.

The objections and reservations raised above should not obscure the value of this publication; it is currently the best general study in English of the Franco regime.

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MARIE-JEANNE TITS-DIEUAIDE. *La formation des prix céréaliers en Brabant et en Flandre au XV^e siècle.* (Centre d'histoire économique et sociale.) Brussels: Éditions de l'Université de Bruxelles. 1975. Pp. xliv, 406. 1,545 fr. B.

For over seventy-five years, economic historians of preindustrial Europe have tried to understand grain prices. Few topics are more technical, but fundamental interpretations of late medieval and early modern economic history are tied to theories and statistical methods dealing with prices. Marie-Jeanne Tits-Dieuaide's study of grain prices in fifteenth-century Brabant and Flanders advances an interpretation at variance with generally accepted views. The author is well aware of this and establishes every point with clarity and massive documentation.

The examination of short-term price movements indicates that Flanders and Brabant were part of a large international grain market stretching from "Utrecht to Rouen." Within this zone, the demand of the large urban centers of Holland, Zeeland, Flanders, and Brabant was paramount. Supplies flowed from the Netherlands, northern France, England, and, most important, from the Baltic. Between the Rhine and the Seine, grain prices everywhere followed basically the same cyclical trends. The forces of the international grain market equalized prices and largely neutralized the impact of local harvest fluctuations. Baltic grains in particular had a tremendous impact not only on the prices found in the cities of Holland where the Polish and Prussian rye was consumed, but also on prices far away in Brabant and Normandy. An interruption of Baltic supplies had the impact of a stone falling on still water: the ripple effects spread out in all directions.

This picture of grain prices in Brabant and Flanders corroborates the recent findings of Van der Wee and Morineau. Serious doubts are raised about the validity of such standard notions as Gregory King's law concerning harvest fluctuations and prices, the closed nature of regional agrarian economies, the minor role of trade in grains, and the causal links between local harvest failure and demographic subsistence crises.

This monograph is a substantial contribution to what may be called a new generation of Continental scholarship on grain prices. The main strengths of this new school are a solid understanding of market mechanisms and economic theory. Within the last decade or so, case studies have multiplied. The new evidence no longer fits the interpretive schema elaborated during the first third of this century. The author provides a valuable review of the major literature on grain prices and a critique of the various statistical techniques currently in use. She concludes her study with a point-by-point examination of accepted views and suggests the main outlines of a new interpretation.

JAMES L. GOLDSMITH
*University of Oklahoma*

L. DE JONG. *Het Koninkrijk der Nederlanden in de Tweede Wereldoorlog* [The Kingdom of the Netherlands in the Second World War]. Volume 6, part 1, *Juli '42-Mei 43.* (Rijksinstituut voor Oorlogsdocumentatie.) s'-Gravenhage: Martinus Nijhoff, 1975. Pp. viii, 514.

This volume covers the period from the start of the deportations of the Jews in July 1942 to the country-wide strikes in April and May 1943. The first part deals with the deportation of the Jews, the growth of the Resistance movement, and the entire area of ideologically inspired collaboration.

As in earlier volumes, the general picture that L. De Jong presents for this period shows no startling new revelations. The entire German apparatus carried out the removal of the Jews with deception and utter ruthlessness. Along with the forced labor draft, the deportation contributed significantly to the enlargement of the Resistance movement since a number of groups were formed to support persons who went into hiding. Also during this period it became apparent that the Dutch Nazi movement and the organizations and institutions established on the German model had completely failed to convert a significant segment of the Dutch people to National Socialism.

De Jong, however, furnishes new information and insights regarding many specific topics. He shows the great extent to which the Dutch administration and police helped the Germans carry out the deportation of the Jews. He points out how few Dutch officials refused to participate in the anti-Jewish measures, and that those who did were not harassed or arrested. He reports that *Reichskommissar* Seyss-Inquart usually sided with the radicals of the SS in refusing to exclude certain categories of Jews from the deportation. De Jong also indicates that the Germans paid ninety percent of the expenses of the N.S.B. (the Dutch Nazi movement), and that the Dutch Nazi leadership profitted heavily from their offical positions even where they did not break the law through outright corruption.

In this volume, as in the preceding ones, one cannot but admire the detail, the scope of information, and the author's good judgment and the lively style. At the same time I wonder whether the main line of the story becomes obscured by this much detail, whether broad general assessments are made in a manner easily accessible to the

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reader, in short whether he misses the forest for the trees.

WERNER WARMBRUNN
*Pitzer College*

KNUD LARSEN. *Forsvar og Folkeforbund. En studie i Venstres og det konservative Folkepartis forsvarspolitiske meningsdannelse, 1918–1922* [Defense and the League of Nations. A Study in the Formation of Opinion in the Defense Policies of the Liberal and Conservative Parties, 1918–22]. Summary in English. (Jysk Selskab for Historie, number 31.) Aarhus: Universitetsforlaget. 1976. Pp. 453. 88 KR.

As in many other countries, there was a desire in Denmark to reduce military expenditures immediately after World War I. Knud Larsen examines the debate which led to the establishment of a new military budget in 1922. This study, a doctoral dissertation at the University of Copenhagen, focuses on those two parties, the Liberals (*Venstre*) and the Conservatives, most responsible for the new budget.

Larsen's subject is an important one because the arrangement of 1922 became the basis for Denmark's military preparedness until World War II. Coverage of the years 1922–1940 would also have been interesting, however, since there is no satisfactory study of this period. The book contains many valuable insights, but it is burdened by a considerable amount of deadwood which could have been removed and replaced with pertinent information on the subsequent period.

The author launches into his subject without any background information. This is quite frustrating, especially to non-Danish readers, and more so because speakers during the parliamentary debates make references to previous situations that Larsen does not otherwise mention. Serious questions can be raised about the whole organization of the work, as the author himself admits in the introduction. His approach is to consider various aspects of the subject: the parties, the League, Denmark's potential adversaries, the military experts, and finally the political compromise. The result is that the relative importance of the various factors influencing the compromise becomes obscured. Furthermore, important factors have been ignored. There is no consideration of the role of public opinion; and a thorough discussion of Denmark's actual potential to defend itself in terms of manpower, geographical factors, and technological questions would have done much to clarify the issues.

While there is no doubt that the author has considerable potential as a historian, the present work leaves much to be desired. Larsen tends to be redundant, and he sometimes pursues avenues more for the sake of demonstrating his competence as a researcher than in order to provide significant answers.

WILLIAM D. ANDERSEN
*University of Wisconsin, Madison*

H. ARNOLD BARTON. *Count Hans Axel von Fersen: Aristocrat in an Age of Revolution.* (The Library of Scandinavian Studies, volume 3.) Boston: Twayne Publishers. 1975. Pp. 530. $12.50.

Count Hans Axel von Fersen (1755–1810) is probably best known for helping the French royal family in their dramatic 1791 escape attempt to Varennes. H. Arnold Barton has subtitled his comprehensive study of the Swedish nobleman, "Aristocrat in an Age of Revolution," which aptly sets the tone for the book and its concentration on Fersen's political significance. Axel von Fersen was first and foremost an aristocrat, brought up in the old tradition of Swedish politics in which the nobles served as a loyal opposition in running the government. Fersen, however, rose above the traditional tension between crown and nobility, of which his father was a prime representative, when he supported the monarchy (be it Swedish or French) during the stressful time of the eighteenth-century revolutions. Maintenance of the old system was reason enough for bridging traditional enmities. And, although Barton feels that Fersen's aid to Marie Antoinette was motivated primarily by a deep personal love, he makes an equally good case for its foundation in Fersen's political ideology.

Fersen's world view permeates Barton's book, and so the emphasis lies much more heavily on "aristocrat" than on "revolution." Barton has gotten to know his subject so well that he seems actually to be able to see the world through Fersen's eyes. Thus coverage of revolution is scanty, except insofar as it impinges on Fersen's life and activities, and then only so far as Fersen would have understood it. One can discuss whether a biographer more rightly ought to write from the perspective of his subject or provide an overall view of his subject's age; Barton has chosen the former course and in that has succeeded admirably well. The reader comes away from the book with a feeling of perplexity about these strange new forces of rebellion, a feeling akin to what Fersen's own reaction must have been.

One should not, therefore, seek an explanation of "the age of revolution" in Barton's book, but rather a very thorough and detailed delineation of one of its prime opponents. Barton feels that Fersen's "greatest historic significance lies in the po-

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litical sphere" (p. 401). Despite the fact that he so completely epitomized a time that no longer existed, he nonetheless had a practical program for the future. This program for restoration accepted the fact that the past, as it had been known, was unrecallable. Barton's contribution, beyond that of providing a more complete and objective study of Fersen than has previously been available, lies in the degree to which he has been able to make his subject of vital significance.

CAROL GOLD
*University of Minnesota, Twin Cities*

ANN-KATRIN HATJE. *Befolkningsfrågan och välfärden: Debatten om familjepolitik och nativitetsökning under 1930-och 1940-talen* [The Population Question and Public Welfare: The Public Debate on Family Policies and Increased Fertility during the 1930s and 1940s]. (Sverige under andra världskriget.) Stockholm: Almänna Förlaget. 1974. Pp. 284.

Doctoral dissertations continue to pour forth from the SUAV (Sweden during World War II) project in Stockholm. At last count the total number was eighteen. Seldom has such a large research project been so productive so quickly. Officially all the work focuses on the war period; in reality there is considerable chronological latitude as in the study under review here which covers the period 1934–1946. The quality of the dissertations has varied enormously, and some have caused considerable public debate. The major problem with the project as a whole has been the rather unimaginative way dissertation topics have been parceled out. The tendency has been to be quite catholic in divisions of labor—separate studies, for example, on each of the various ministries, each of the political parties, some special issues, etc., but no integrated analysis of the governmental decision-making process. Each study offers its own little piece to a puzzle that may not be complete even when the project is finished.

Ann-Katrin Hatje's *Befolkningsfrågan och välfärden* is one of the least typical and most interesting dissertations in the project. The subject broadly defined—family policy and the role of women—has great contemporary interest, and the period under examination was a pioneering one both from the viewpoint of the subject and for the development of the Swedish welfare state. It has been long understood that the population debate that surfaced after the publication of Alva and Gunnar Myrdal's *Crisis in the Population Question* (1934) served as a valuable vehicle to build a broad political consensus for future social reform. Hatje focuses primarily on the work of the two special governmental commissions appointed in 1935 and 1941 to study the problems and the debates that resulted. She offers special sections on the debates surrounding the cost of child rearing, abortion, women as child producers, women as a worker pool, and the differences in principles and solutions offered by the various political parties. The book has an eight-page English summary.

What does the author conclude? She suggests that there were considerable differences of opinion both within the political parties and between them. Men dominated in Sweden, and the debate in the thirties, despite active participation by a few women, could be characterized as a paternalistic discussion of the role of the family in society. Swedish birth rates in 1934–35 were the world's lowest. Who would populate Sweden in the future, and how much responsibility should the state accept for the support and education of children? These questions were primary although the Social Democrats were interested in the issue largely as part of a greater concern to increase equality between social classes. The Conservatives, who had first raised the problem of declining population, showed themselves willing to accept considerable social reform in this area. Despite differences in attitudes and goals, a broad parliamentary consensus could be built for numerous reforms in the family-support area. Hatje concludes that the Social Democrats wanted vertical reform (i.e. decreases in class differences), whereas the Conservatives supported horizontal changes (i.e. the elimination of the disadvantages of large families vis-à-vis small within each class), and that the adopted program tried to satisfy both positions.

The book is not a social historical study of the family or of the role of women in Swedish society. Rather it is an analysis of the public debate on these issues. One is struck by the deep conservatism of the Swedish discussion. The great concern for racial purity even among some Social Democrats, the principal view of woman as the Great Mother, the unwillingness to remove restrictions on the dissemination of birth-control literature, attitudes toward abortion and unwed mothers—all remind us that Sweden of the 1930s and 40s was no progressive paradise. Sweden struggled with problems and attitudes that were very much in the mainstream of the Atlantic experience.

STEVEN KOBLIK
*Pomona College*

HANS-ULRICH WEHLER, editor. *Der deutsche Bauernkrieg, 1524–1526.* (Geschichte und Gesellschaft: Zeitschrift für Historische Sozialwissenschaft, number 1.) Göttingen: Vandenhoeck and Ruprecht. 1975. Pp. 356.

This first in a series of occasional volumes spon-

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sored by the journal *Geschichte und Gesellschaft* comprises eleven essays that demonstrate the application of sociological methods to historical materials. Heide Wunder combines historical and anthropological methodologies to reassess the tradition of mindless peasantry east of the Elbe. John C. Stalnaker, seeking a socio-historical interpretation, stresses economic bases and calls for thorough study of sixteenth-century German peasantry. Rudolf Endres finds that oppression through taxation by local rulers stimulated selective hatred against privileged nobles and monks.

Rainer Postel proposes a methodology for analysis of key urban and rural factors on a regional level. Horst Buszello finds urban-rural parallels in demands for local autonomy, for class-based regimes, and ultimately for a new German political order. Thomas S. Sea concludes that the Swabian League exercised judicial and punitive powers effectively, but local backlash damaged league unity.

Jürgen Bücking posits large-scale changes in feudal agriculture, society, class-structure, and law that were reflected in the revolts through issues of finance, leadership, demands, and casualties. Heinz Schilling, in a highly theoretical investigation, finds that Münster during the period 1525–34 fits a model of constitutional/juridical relationships in which urban unrest in old Europe reveals power struggles within ruling elements. Otthein Rammstedt seeks to establish an existence for urban revolts independent of the peasants in the articles of cities not in peasant-activated areas, notably Frankfurt am Main. Winifred Schulze details a theory that the sixteenth- and seventeenth-century conflicts provided some real social-political negotiating ground between powerholders and rebels.

Franklin Koptizsch and Rainer Wohlfeil produce an extraordinarily comprehensive review of "*Neue Forschungen zur Geschichte des deutschen Bauernkrieges*," including strong critiques of Marxists.

KYLE C. SESSIONS
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HELGARD ULMSCHNEIDER. *Götz von Berlichingen: Ein adeliges Leben der deutschen Renaissance*. Sigmaringen: Jan Thorbecke Verlag. 1974. Pp. 317. DM 48.

Götz von Berlichingen has lived two lives, that created for him by Goethe in 1771—the freedom-fighter and hero of the peasants' revolt—and his real life, told with admirable precision and insight in this new scholarly biography. The "man with the iron hand" lived a colorful life as a mercenary and as an independent military leader. He was enmeshed in the fall of Duke Ulrich of Württemberg and engaged in a decade of feuds with Cologne, Nuremberg, and Mainz. He became involved in the peasants' war, fought in two imperial wars, was placed under the imperial ban four times, and when nearly fifty was interned in his castle at Hornberg, from whence he initiated endless legal processes to regain his freedom and lands. He died in 1562 at the age of eighty-two, one of the most remarkable figures of the Reformation era.

Recent research shows that the imperial knights played an important part in constitutional and social development in Franconia. The role of the nobility in general in sixteenth-century history is receiving new recognition. But general works, such as the badly needed comprehensive history of the Swabian League, for example, cannot be written before the publication of more genealogies (a field neglected since the eighteenth century), of detailed descriptions of land holdings, and of prosopographic and individual biographical studies. Biographies are badly needed, for, aside from Franz von Sickingen and Ulrich von Hutten, an army of fascinating figures of the knightly class, from the highly cultured courtier Sebastian von Rotenhan to that conscienceless thief Hans Thomas von Absberg, awaits serious study.

This present volume on Götz may well serve as a model, for it reflects seven years of painstaking research in twenty-three archives and it is skillfully written. One might wish that the author would have ventured some further judgments on the broader questions of class role in the Reformation, the adaptation of the nobility to the new culture, and the economic predicament of the order. It is unlikely, however, that this biography will be superseded unless by some good fortune Götz's own cache of carefully ordered private papers, which he sent away to Count Michael von Wertheim in 1552 for safekeeping, is rediscovered.

LEWIS W. SPITZ
*Stanford University*

KLAUS SCHWARZ. *Die Lage der Handwerksgesellen in Bremen während des 18. Jahrhunderts*. (Veröffentlichungen aus dem Staatsarchiv der Freien Hansestadt Bremen, volume 44.) Bremen: Selbstverlag des Staatsarchivs der Freien Hansestadt Bremen. 1975. Pp. 401.

In recent years a number of scholars have seen the artisan culture of the late eighteenth and early nineteenth centuries as one charged with a progressive revolutionary potential. The threat of proletarianization posed by accelerated economic development drove artisans—especially propertyless journeymen—from a defense of traditional social relations to an assertion of general human values

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and toward a fundamental critique of the emerging capitalist system. With variations this thesis has been advanced for England, America, France, and Germany by writers of both Marxist and non-Marxist persuasion.

Klaus Schwarz reverts to a more traditional interpretation. Incapable of meeting the material needs of an expanding population, Bremen's guilds nonetheless continued to insist on their traditional economic and social privileges, stubbornly attempting to preserve their world as it had existed a century earlier. Moreover, journeymen as a class continued to support the old system, despite the fact that they suffered a steady proletarianization after 1740, as real earnings declined and it became progressively more difficult to rise to the position of master. For Schwarz this "crass contradiction" between consciousness and material conditions demonstrates the bankruptcy of the guild order, and shows that their elimination was nothing more than a "precondition for further economic and social development."

Unfortunately the limits imposed on the study undermine the interpretation. Schwarz accepts legal change too uncritically as representing social transformation, and too easily allows the organization of the archives to determine the form as well as the content of the analysis. To end a study of this highly conscious and traditionalist social group with the three years of French administration (1810–13) is implausible and omits precisely the most significant period of pressure on artisan institutions and culture. The refusal to utilize comparative data from Germany and elsewhere leaves dark much of what might have been illuminated by the study.

However, though Schwarz's work shows some of the weaknesses of the cautious, narrowly focused local study, it reflects the strengths of the genre as well. The carefully documented results are solid (prices, wages, unemployment), sometimes intriguing (the accommodations by which the city council defused journeyman discontent in the 1790s), and occasionally surprising (nearly three-quarters of the journeymen were married). They will prove useful to those undertaking the more theoretical comparative study he has neglected.

FRANK B. TIPTON, JR.
*Wesleyan University*

W. O. HENDERSON. *The Rise of German Industrial Power, 1834–1914*. Berkeley and Los Angeles: University of California Press. 1976. Pp. 264. Cloth $14.00, paper $3.45.

My colleagues, who spotted this book on my desk during the last several weeks, almost invariably exclaimed, in effect, "My! That must be an important book"—to which my standard response was, "It could have been."

The author, emeritus Reader in International Economic History in the University of Manchester, has had a long and prolific career as an expositor of British, European, and especially German economic history of the last two centuries, with major emphasis on the nineteenth. His many books include *The Zollverein* (1939), *Britain and Industrial Europe, 1750–1870* (1954), *The State and the Industrial Revolution in Prussia, 1740–1870* (1958), *Studies in German Colonial History* (1962), and *Studies in the Economic Policy of Frederick the Great* (1963), in addition to several translations and numerous articles on similar subjects. In a sense, the volume under review might be regarded as the culmination of his life's work; but it is definitely anticlimactic.

W. O. Henderson takes a narrative approach and follows a strict tripartite chronology: "The Dawn of the Industrial Era, 1834–1851"; "The Rise of the Great Industries, 1851–1873"; and "The Industrial Giant, 1873–1914." He recounts all of the standard topics: the formation of the Zollverein; the beginnings of the railways and their consequences for industrial expansion, especially coal and iron; the revolutions of 1848; the boom of the fifties and the "banking revolution" (considerably overdone); "the struggle between Prussia and Austria for economic supremacy" (it was no contest from the beginning); Bismarck and unification; the *Gründerjahren* of the early seventies; the rise of the cartels and of new industries (chemical, electrical, and shipbuilding); and the inception of the welfare state. Throughout, as this recapitulation suggests, Henderson relies on political history as a framework for the narrative and correspondingly emphasizes economic policy. He is generally laudatory of the economic role of the state and, in consequence, overstates its importance. He is mildly critical of the "stringent mining laws" of the first half of the century in Prussia (p. 78), and subsequently (p. 132) refers briefly to their reform; but nowhere does he explicitly mention the *Direktionsprinzip*, by which the state nearly strangled the mining industry, and its replacement after mid-century by the *Inspektionsprinzip* (which Schmoller contemptuously called "Manchesterdom"), an event of fundamental importance for the expansion of the Ruhr coal industry and thus for "the rise of German industrial power."

As a counterpart to his overemphasis on political history, Henderson totally neglects economic analysis. Causal relations are introduced as "factors" without any attempt to weight them according to their relative importance. Statistics abound in the text, but he presents them in an unsystematic fashion, without terms of reference or comparison,

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thus rendering them virtually meaningless. Although there are several excellent maps at the beginning of the volume, the book does not contain a single statistical table. Most annoying of all, there are no footnotes or citations of sources—not even for direct quotations. The bibliography (pp. 245–53) indicates that in spite of his long familiarity with the field, Henderson has relied almost entirely on secondary sources, and mostly dated ones at that.

Some flavor of the style, a mixture of trivial detail with banal generalization, can be sampled in the following: "The main causes for the outbreak of revolution in Germany in 1848 were the urgent need for political reforms and serious economic and social distress" (p. 80); "In Nassau parts of a railway line were destroyed by carters whose trade was declining, while Metternich's wine cellar at Johannisberg was confiscated by insurgents . . . . In Bavaria the refusal of the king to dismiss his mistress Lola Montez sparked off disturbances in Munich, while in Posen the Poles attacked the local German population" (p. 82); "Electricity could be used far from the power station but steam-power could be used only quite near the steam-engine" (p. 191). Non sequiturs abound: "in 1860 the Anglo-French treaty of commerce provided for a substantial reduction in the level of many French import duties. These reforms enabled goods to be exchanged more freely in many parts of Europe and fostered the economic expansion of the 1850s" (p. 113).

Students and teachers of German and European history, especially economic history, have long felt the need for a comprehensive, analytical treatment in English of the rise of German industrial power. They will continue to do so.

RONDO CAMERON
*Emory University*

STANLEY ZUCKER. *Ludwig Bamberger: German Liberal Politician and Social Critic, 1823–1899.* Pittsburgh: University of Pittsburgh Press. 1975. Pp. xi, 343. $14.95.

Ludwig Bamberger's career is unusually interesting. Pushed by the events of 1848 into journalism, oratory, and political leadership, he found himself disillusioned by the political immaturity of and dissension among the German masses. He then reconciled himself to becoming a banker in the family tradition, and ultimately rallied to support Bismarck in his struggle to organize part of Germany under Prussian leadership in the sixties. After 1871 Bamberger became one of the most effective National Liberal party leaders in the Reichstag, being active in the adoption of a single currency, the adherence to the gold standard, and the founding of the Reichsbank. It is because of his practical political schooling that Bamberger's apparently inevitable move from support of Bismarck in the seventies to principled opposition to him in the eighties becomes especially significant. Stanley Zucker documents this transition in a very useful way, although he does not completely explain it. He also clearly indicates the major weakness of the German liberals: the lack of any large natural following among the masses. Some of this failing, he thinks, was the fault of the liberals themselves, whose attitude remained not only too academic, but also too narrowly middle class and business oriented. They constantly criticized the workers for not being individualistic enough, but consistently failed to offer them anything that they really wanted. On the other side, the liberals could not tolerate Bismarck's moves, under the basic stimulus of the "Long Depression," to satisfy the interests of the agrarian Junker gentry and the peasants. In a new, rapidly industrializing nation with authoritarian traditions, German political parties tended to be dogmatically ideological, devoted to special interests, and averse to fundamental compromise. The idealistic, democratic liberals were no exception. The book is well written, and the sources are impeccable. It is must reading for anyone interested in the fortunes and failings of German liberalism or the development of German politics in general. The design and printing are excellent, and the editing is good.

J. ALDEN NICHOLS
*University of Illinois, Urbana-Champaign*

PETER HANNS REILL. *The German Enlightenment and the Rise of Historicism.* Berkeley and Los Angeles: University of California Press. 1975. Pp. x, 308. $18.00.

HEINRICH VON TREITSCHKE. *History of Germany in the Nineteenth Century: Selections from the Translation of Eden and Cedar Paul.* Edited with an introduction by GORDON A. CRAIG. (Classic European Historians.) Chicago: University of Chicago Press. 1975. Pp. xxix, 411. $16.00.

Heinrich von Treitschke, along with many other German historians of his generation, used history as a weapon in the ideological struggle for a united Germany. For these historians, the eighteenth century was a time of national shame and (except for the literary revival) sterility. Treitschke himself, in praising the idealism, religiosity, nationalism, and romanticism of Germany in 1815, belittled the sensualism, rule of reason, cosmopolitanism, and natural-law theory characteristic of the preceding age of Enlightenment. He also characterized the nineteenth century as the age of "historico-aesthetic"

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(as opposed to "scientific") culture. Historians of the Enlightenment fared equally badly, being dismissed as Frenchified dabblers, superficial encyclopedists unable to think in the characteristic terms of nineteenth-century German historicism.

This overdrawn image is the target of Peter Hanns Reill's book. Not only does he argue that the Enlightenment (or rather the German *Aufklärung*) was not antihistorical, he goes so far as to locate the birth of historicism in the *Aufklärung*: "If. . .the rise of historicism is one of the great intellectual revolutions of the modern age, then the larger part of this intellectual revolutionary activity was carried out in the Enlightenment by Enlightenment thinkers" (*German Enlightenment*, p. 220). Reill's argument is elaborate and bolstered by wide reading in the works of over a score of "Aufklärers" (as he persistently calls them). These historians developed a somewhat different viewpoint and method from such foreign historians as Montesquieu and Hume, owing to three major components of the German *Aufklärung*: Leibnizian philosophy, the peculiarities of *Ständestaat* politics, and religiosity. Despite undeniable influence from abroad, the German Enlightenment had a homegrown quality based on the peculiar social grounding and intellectual traditions of the German historians.

Naturally a study of so many historians writing over a full century requires some organizational themes to maintain coherence. The author dwells in particular on categories of historical causation, the origins of humanity, and laws of development. Despite this interjection of themes, the work often requires careful reading to discover the main points. Many of the chapters begin and end without extensive statements of hypothesis or conclusion. Another weakness of the book is that the historians treated—such as Arnold, Semler, Schmauss, Iselin, or Chladenius—were not intellectual giants (as Reill readily admits). Yet the method used to study them is the same traditionally applied to great thinkers. This sometimes leads to tedious reading. At the same time, the social and institutional setting of these workaday intellectuals, about which it would be interesting to know more, receives scant attention. Despite these various faults, however, and despite the inadequate persuasiveness of the central hypothesis on the origins of historicism in the Enlightenment, the work does rescue from undeserved obscurity a host of writers who aided greatly in the modernization of German historical thinking long before Niebuhr and Ranke.

Treitschke does not require rescue from obscurity, but as Gordon Craig points out, most English-speaking readers know him primarily from his *Politics* rather than from this *chef d'oeuvre*. This condensation from the 1914–19 translation (in seven volumes) by Eden and Cedar Paul should revive interest in one of Germany's great historical stylists. Few today will sympathize with Treitschke's Prussian monarchist bias or his didactic purpose of bridging rhetorically the deep fissures in German society. But one can well appreciate the reasons for Treitschke's conversion to nationalism by reading his description of the Saxon court and *Kleinstaaterei*, which made such a deep negative impression on this son of a Saxon general. This was a work of "passion and love" (p. xiii), taking up arms against the "bloodless objectivity" of other German historians, whose partisanship was blander or better concealed. For all his bias, Treitschke was also a hard-working researcher, and this volume contains many useful fruits of his industry.

Craig has selected from seven volumes with sure skill, choosing to emphasize more engaging areas such as literary developments over the more detailed and specialized parts concerning, for example, the workings of the Germanic Confederation. He has also contributed an excellent brief introduction.

CHARLES E. MCCLELLAND
*University of New Mexico*

DIRK H. MÜLLER. *Idealismus und Revolution: Zur Opposition der Jungen gegen den sozialdemokratischen Parteivorstand, 1890 bis 1894*. (Historische Kommission zu Berlin. Beihefte zur Internationalen wissenschaftlichen Korrespondenz zur Geschichte der deutschen Arbeiterbewegung [IWK], number 3.) Berlin: Colloquium Verlag. 1975. Pp. viii, 186. DM 44.

Dirk Müller's book provides the first in-depth treatment of the "*Opposition der Jugend*"—those Social Democratic elements that attacked the party leadership for its nonrevolutionary policy at the beginning of the 1890s. The most vocal of these critics were generally in their twenties and early thirties; hence the label "*Jugend*." In 1892, this opposition split off from the Social Democratic Party (SPD) to form the short-lived Union of Independent Socialists (VUS).

The *Opposition der Jugend* was an important symptom of deep tactical and strategic differences in the Social Democratic organization at a crucial juncture in its history. Bismarck and his antisocialist legislation had passed from the scene, and the party leadership was preparing a new organization and program to correspond to the changed political situation. For the *Jugend*, however, neither the organizational statutes nor the Erfurt program were satisfactory; they condemned the former as dictatorial and attacked the latter for failing to

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break with reformism. Dissatisfaction with the centralization of the party structure and the emphasis on parliamentary activity was not limited to the *Jugend*, as the May 1 strike movement in 1890 demonstrated. Yet the *Jugend* were unable to harness this discontent and provide a viable revolutionary alternative. There were two reasons for this, both related to the non-Marxist, idealist orientation of the *Jugend*. On the one hand, they tended to personalize their criticism, accusing individuals of moral betrayal, thus obscuring the real political issues. On the other hand, outside of a kind of revolutionary voluntarism, they offered no solution to the conflict between fatalist revolutionary theory and reformist practice that characterized the Erfurt program. As a result, the *Jugend* had a limited following, especially after they were forced out of the SPD. The VUS proved to be little more than a discussion group, and its only practical impact may have been to weaken the resistance to reformism within German Social Democracy.

Müller has presented a well-researched and differentiated analysis of the *Opposition der Jugend*, albeit a book for the specialist. Not, for example, until forty-seven pages into the study does the uninitiated reader discover why the *Jugend* are so called. The book overflows with detail and citations, some of which tend to confuse more than illustrate or clarify. No doubt the expert will have little trouble working his or her way through this work, but the nonspecialist may wish for more guidance.

ROBERT F. WHEELER
*University of Southern California*

RICHARD S. LEVY. *The Downfall of the Anti-Semitic Political Parties in Imperial Germany*. (Yale Historical Publications, Miscellany, number 106.) New Haven: Yale University Press. 1975. Pp. vii, 335. $18.50.

At the heart of this excellent monograph lies a fundamental distinction between "conventional" and "revolutionary" anti-Semitism. Representatives of the former, men such as Adolf Stöcker, Otto Böckel, and Liebermann von Sonnenberg, sought to gain their ends through the organization of political parties working within the parliamentary framework of imperial Germany; advocates of the latter, for example Theodor Fritsch and Eugen Dühring, believed that the Jewish peril could be fully overcome only in conjunction with an overthrow of existing institutions. Richard S. Levy is concerned almost exclusively with conventional anti-Semitism, which he traces from its origins in the aftermath of the 1873 depression to the eve of World War I. Although both Paul Massing and Peter Pulzer have written full-scale interpretations of political anti-Semitism in the *Kaiserreich*, the present volume provides a far more detailed and probing account of the specific circumstances leading to anti-Semitic successes and failures. Drawing upon the *Mitteilungen* of the *Verein zur Abwehr des Antisemitismus* as his most valuable single source, Levy has been able to reconstruct local and regional developments as well as the anti-Semites' national politics. His discussion of the movement in Hesse is particularly enlightening.

The title of the volume indicates its thesis. Levy argues that conventional anti-Semitism in imperial Germany was, on the whole, a dismal failure. The anti-Semitic parties' final aim was to reverse the emancipation of the Jews, a step requiring the amendment of the constitution. Reaching a maximum representation of seventeen in the Reichstag of 1907, they pursued a hopeless goal. Repeatedly their cause was damaged by the need to make overtures to conservative parties not "sincere" in their anti-Semitism, by internal dissension over economic and other non-anti-Semitic issues, and by scandal. Levy shows that their two moments of relative electoral success, in 1893 and 1907, resulted in each instance from temporary fortuitous circumstances.

The book raises anew the question of a link between Wilhelminian and Weimar anti-Semitism. According to Levy, there was indeed a connection, but it was with the revolutionaries, not with the politicians. The Nazis venerated Fritsch but scorned the efforts of the conventional anti-Semites. The tragic error of the defense organizations was to believe that the tactics of rational apologetics, which proved quite effective against a conventional anti-Semitism concerned with respectability, would work as well against a ruthless and revolutionary Nazism. Though Levy's thesis is subject to some qualification, he argues it forcefully and cogently and with verbal precision.

MICHAEL A. MEYER
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JOHN K. ZEENDER. *The German Center Party: 1890–1906*. (Transactions of the American Philosophical Society. New Series, volume 66, part 1.) Philadelphia: the Society. 1976. Pp. 125. $7.50.

The Center Party, the political expression of German Catholicism, pursued "basically defensive" aims; accepting monarchism and fearing the anticlericalism of a potential democratic state, the party sought to defend the rights of Catholics within the existing structure of the German em-

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pire. John K. Zeender has intensively studied the political correspondence of leading Centrists. His detailed, meticulous monograph deals with the actions of the Center's Reichstag leadership and comes close to being a biography of Ernst Lieber, caucus chairman in the 1890s.

The Center Party, itself divided on economic and ideological issues, met rejection from the imperial establishment despite the sincere longing of Catholics to prove their loyalty as good Germans and monarchists. Zeender sympathetically, but not uncritically, explains the complex, even tortuous course of Centrist policy. After Ludwig Windthorst, Bismarck's old foe, died in 1891, the Centrist leadership sought to win official respect for the Reichstag and its largest party, the Center, by meeting more than halfway Wilhelm II's demands on the "national" (army and navy) issues. The Center, Zeender concludes, accepted Admiral Tirpitz's naval program as an implicit trade in return for a more benevolent official policy toward industrial workers. The agricultural tariff of 1902, which the Center helped ram through against Socialist obstruction, reassured Prussian Protestant conservatives and Bavarian Centrists that close cooperation between the Center and the government was not hurting their particular interests. Thus the Center became a key partner of the imperial government.

Although a traditional legislative history in its sources and methodology, Zeender's work is an important, revealing study of a major party. Only briefly and somewhat obliquely does Zeender consider the wider responsibility of the Center for Germany's constitutional impasse and international isolation.

JAMES C. HUNT
*Saint Joseph's College, North Windham, Maine*

FRITZ BLAICH. *Der Trustkampf (1901–1915): Ein Beitrag zum Verhalten der Ministerialbürokratie gegenüber Verbandsinteressen im Wilhelminischen Deutschland.* Berlin: Duncker and Humblot. 1975. Pp. 157. DM 39.60.

This volume tells the story of the German reaction to a foreign trust, James B. Duke's American Tobacco Company, and its successor, the British-American Tobacco Company. Like Fritz Blaich's previous book, *Kartell- und Monopolpolitik im kaiserlichen Deutschland* (1973), it thus deals with that political-economic transformation variously labelled state monopoly capitalism, organized capitalism, and corporatism. Blaich's work is a counterpoise rather than a contribution to these theories, however, for he emphasizes the wide-spread hostility to economic concentration among the industrial *Mittelstand*.

The trust was the ultimate threat to economic pluralism. It was denounced by bureaucrats, businessmen and academics as a pernicious American innovation, not to be confused with decent German cartels. The American trust, operating through the Jasmatzi group, never controlled more than 27% of German cigarette sales, compared with 86% of the U.S. market, but its presumed ambitions and novel methods (coupons redeemable for merchandise, heavy investment in mechanical production, adoption of new retailing practices) disrupted the industry and frightened local firms. Some imitated these tactics, but the easiest defense was an attempted boycott of trust brands. Especially vital were military posts, where brand loyalty was often formed, and railroad station shops, which were exempted from evening and holiday closure laws. The antitrust forces tried repeatedly, and with some success in the Bavarian army, to have their rivals' cigarettes barred from sale in canteens. This was not a nationalist crusade, but a calculated effort to avoid competition. It was also expected to mobilize bureaucratic support against the "American danger." Most officials were not impressed by "patriotic" propaganda, however, and Blaich defends them against charges of favoritism.

As the *Trustkampf* gathered momentum after 1910, it created a snarled web of financial problems and opportunities for the Reich, Saxony, and the city of Dresden, the center of cigarette manufacturing. Officials in Berlin, weary of the conflict and desperate for revenue, drew up a plan for a national tobacco monopoly and seriously considered buying out the trust in December and January 1914–15. The story, generating considerable suspense, builds to a fitting climax: the sale of the trust to the Deutsche Bank.

Blaich's reluctance to discuss concepts like organized or state monopoly capitalism is disappointing, but he provides basic, and fascinating, information for anyone trying to understand the peculiarities of modern Germany.

CHARLES MEDALEN
*University of Arizona*

REINHARD R. DOERRIES. *Washington—Berlin, 1908–1917: Die Tätigkeit des Botschafters Johann Heinrich Graf von Bernstorff in Washington.* Foreword by R. VIERHAUS. Düsseldorf: Pädagogischer Verlag Schwann. 1975. Pp. 299. DM 38.

The events leading to the entry of America into World War I against Germany have been described repeatedly by historians in both countries. This latest contribution by a Hamburg historian centers on one of the leading actors in the diplomatic drama, Count Johann Heinrich von Bernstorff, German ambassador to Washington from

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1908 to 1917. Taking full advantage of the unpublished German foreign ministry documents, and combing American libraries and archives, Reinhard R. Doerries uncovers some fresh information, though on the whole his book confirms those earlier accounts that have been critical of German policy. Only one of the six chapters deals with the prewar years. Three of the remaining chapters discuss, chronologically, the role of U-boat warfare in bringing about American intervention, and the concurrent efforts, mainly of Count Bernstorff and Colonel House, to avoid such intervention by a negotiated peace. The author does not attribute to the U-boat campaign the "decisive importance" that most other authors give it; but he never clearly suggests his own alternative interpretation, unless it be that amateurish German propaganda efforts, involvement in various acts of sabotage, and fomenting of revolutions helped to undercut much of the ambassador's more constructive work. Doerries devotes his longest chapter to the activities of various German agents, secret and otherwise, in what the Germans euphemistically called "propaganda of the deed" (*Propaganda der Tat*), namely sabotage. He takes these shenanigans more seriously than have other writers and criticizes Bernstorff for his "silent acquiescence" in acts that tended to undermine his more positive policies. Yet on the whole, Bernstorff emerges as a decent, moderate, and able member of the German diplomatic establishment who shared the fate of the few other sensible Germans in having his advice ignored and his moderation maligned by his more chauvinistic compatriots. All in all, this is an interesting and readable book.

HANS W. GATZKE
*Yale University*

JOHN A. MOSES. *The Politics of Illusion: The Fischer Controversy in German Historiography*. New York: Barnes and Noble. 1975. Pp. xvii, 148.

Fritz Fischer, once more. The Germans did it. Theirs was the chief responsibility for World War I, both as far as the long-range causes and the immediate origins were concerned. The whole trend of a militarized German society—supported in no small degree by the excessive regard for the state, the nationalism and expansionism of the "Neo-Rankean" school of historians—for years had aimed at war. And ever since the emperor's War Council of December 8, 1912 (a sort of early Hossbach meeting) German policy was purposely and directly moving toward a military confrontation.

It is Fritz Fischer's great merit to have proved imperial Germany's guilt beyond any reasonable doubt, or so we are told by John A. Moses, a German-trained Australian scholar, in this summing up of the controversy over Fischer's second book, *War of Illusions*. Nor is this Fischer's only contribution. By his utter honesty, by "revealing the illusions upon which imperial German policy was based," he and the writers influenced by him have been doing nothing less than "creating the ideological-political pre-condition for a durable democratic order in Central Europe."

The case is certainly worth making, and Moses is as well informed as anyone on the course of the Fischer controversy. This is true even though some of his translations from the German are a bit quaint; one would feel happier about an author who did not translate "*Betriebsunfall*" as "accident in the works." But that point may be as petty as it sounds; Moses clearly knows his literature.

Why then the sense of unease in reading this book? In part, it must be admitted, the reason is personal. To a reviewer who comes to the study of 1914 from an initial interest in Southeast European history, there is something surreal about a discussion of war guilt which mentions neither Dimitrijević nor Berchtold. But there is something else, purely in terms of the book itself, that is disturbing. It is a tendency to overinterpret, which is a nicer way of saying that on more than one occasion one has a feeling that both Fischer and Moses start with certain assumptions, and then shape the evidence to support them. Thus facts get buried under historical interpretations and reinterpretations; what emerges is not what Bethmann said or did, but how Hillgruber's interpretation of these words or acts differs from Ritter's, and Fischer's from Erdmann's. After being told how Reizler felt that German economic growth must be followed by political growth, we read that "Riezler did not spell out whether he wished this to take place by force, but given his *Weltanschauung* about the nature of power to expand to world domination, it can be assumed that Riezler was aware that this goal could only be achieved by going to war."

But that is not history, it is scholastic argument. And the fact that one writer is exempt from critical analysis makes it less than convincing even as an argument. That writer, of course, is Fritz Fischer, with his "epoch-making *Griff nach der Weltmacht*," and while "it will remain for future generations to assess the full impact of Fischer's historiographical achievement," it already is apparent that "he has taken an all-important step in 'democratising' the historical discipline (i.e., by rejecting the conservative state-veneration of his predecessors) and therewith will democratise the political values of future generations of German students. Indeed, there can be no historiography without ideological commitment."

Perhaps so, but if Moses is right, we will have to remove a good many books from the history lists.

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But there is something truly frustrating about writing a review of anything connected with Fischer and his school. The arguments have all been made, and made again. Neither skeptics nor believers are likely to have a change of mind or heart. So, in this modest mood, two final suggestions only: One is that it at least seems possible that Fischer's ideas, rather than ushering in a new birth of democracy, have merely become the new orthodoxy, which before long will be replaced by another, and quite different, set of ideas. After all, why should the dialectic stop for Fischer or Moses? The other is that when the new stage emerges, we will be lucky to have many readers left, for the debate that sparked so much excitement in the sixties is adding to history's boredom factor in the seventies and is becoming, as scholasticism did, an intellectual exercise without an audience other than one's colleagues. It is time we found a new villain.

JOACHIM REMAK
*University of California, Santa Barbara*

HARTMUT SCHUSTEREIT. *Linksliberalismus und Sozialdemokratie in der Weimarer Republik: Eine vergleichende Betrachtung der Politik von DDP und SPD, 1919–1930.* (Geschichte und Gesellschaft: Bochumer Historische Studien.) Düsseldorf: Pädagogischer Verlag Schwann. 1975. Pp. 296. DM 38.

The object of this book is to examine in a comparative context the policy of the German Democratic Party (DDP) and the Social Democratic Party (SPD) during the first twelve years of the Weimar Republic. Given the involvement of both these political organizations in the first extended German experiment with parliamentary democracy, the topic is an important one. The author's decision to approach the subject in a topical and comparative fashion combined with the wealth of archival resources promises a valuable and interesting contribution. His book, however, is a disappointment.

Hartmut Schustereit's main thesis is never clear. It seems to be that the DDP continually sought to include the Social Democrats in governmental coalitions as a means of insuring their support for the parliamentary republic and "educating" them in their responsibilities to the German state ("die SPD zur Staatstreue zu erziehen" p. 256). An apparent corollary to this was that the DDP was much more concerned with the SPD than vice versa. Yet because of Schustereit's unfocused presentation such basic ideas remain obscured. This problem is compounded by his inconsistency in following the topical approach. Only in the last three chapters (those dealing with civil service policy, educational policy, and the image both parties had of themselves and each other) does he fully implement it. By contrast the first two chapters (which deal respectively with Left Liberal–Social Democratic contacts in late Wilhelmine Germany and DDP-SPD relationships during the first year of the Republic) are strictly chronological and the middle five (Kapp Putsch [1920], Ruhr Occupation [1923], the 1925 presidential election, the Flag Dispute [1926], and the dynastic expropriation plebiscite [1926]) are not quite one or the other. The result is organizational confusion; more often than not the parts are never clearly related to the whole.

This is unfortunate because Schustereit has tapped some impressive archival collections, and his book should provide a valuable source of information for students of Weimar politics. Still, rather than mastering these sources, the author seems imprisoned by them. His book is long on descriptive detail, quotations, and paraphrasing while short on analysis. Indeed, because Schustereit has found incomparably richer sources for a study of the DDP than of the SPD (as he himself confesses in the preface), the work has become not a balanced study of the two parties, but rather an examination of DDP policies toward and perceptions of the SPD.

ROBERT F. WHEELER
*University of Southern California*

KURT G. P. SCHUSTER. *Der Rote Frontkämpferbund, 1924–1929: Beiträge zur Geschichte und Organisationsstruktur eines politischen Kampfbundes.* (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, volume 55.) Düsseldorf: Droste Verlag. 1975. Pp. 290. DM 64.

From several standpoints Kurt G. P. Schuster's dissertation is a welcome addition to the historical literature on the Weimar Republic. It complements studies by Volker Berghahn on the *Stahlhelm,* Karl Rohe on the *Reichsbanner,* and Andreas Werner on the SA, so monographs are now available on all the major political armies of the Weimar period. Schuster's work also deals with an organization that Hermann Weber treated only peripherally in his fine study of the Stalinization of the KPD.

The *Roter Frontkämpferbund* is still a politically sensitive subject in the GDR, and Schuster was not permitted to see important materials there, such as the records of the *Reichsinnenministerium.* He has compensated by piecing together documents exchanged among the political police of various German states, which reveal that the police were remarkably well informed on most aspects of the RFB's organization, activities, and internal disputes.

The emphasis in Schuster's study falls on the

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same features of the RFB that interested the police: its bureaucratic structure and affiliated groups and its factional rivalries. He also provides long chapters on the founding of the RFB and its prohibition in 1929. Schuster shows conclusively that the RFB was never as large or effective as both it and its political enemies asserted, and documents the subservience of the political army to the KPD and how the purge of the opposition within the RFB accompanied Stalinization of the party. Indeed, the author concludes that the purges that followed the Comintern's shift to the left in 1928 so shook the RFB that its prohibition the next year probably saved it from collapse born of its own dissension and demoralization.

While it describes exhaustively the organization and coordination of the RFB, Schuster's study does not make for exciting reading. Scholars must also rely on the other monographs for extended treatment of broader problems such as the doctrinal fluctuations to which the RFB responded and a comparative analysis of the various party armies. Despite its limitations, of which the author is well aware, this valuable book succeeds in filling an important gap in the existing literature.

ROGER CHICKERING
*University of Oregon*

ERNST FEDER. *Heute sprach ich mit . . .: Tagebücher eines Berliner Publizisten, 1926–1932*. CÉCILE LOWENTHAL-HENSEL and ARNOLD PAUCKER, editors. (Veröffentlichung des Leo Baeck Instituts.) Stuttgart: Deutsche Verlags-Anstalt. 1971. Pp. 431. DM 38.

As domestic political editor during 1919–1931 of one of Germany's most respected liberal newspapers, the *Berliner Tageblatt*, and as an important participant in the inner councils of the German Democratic Party (*Deutsche Demokratische Partei*), Ernst Feder (1881–1964), lawyer, journalist, and politician, was an intimate witness to the deterioration of democratic forces in the Weimar Republic. As an assimilated Jew, intellectually sophisticated and professionally successful, he lived a comfortable and exciting life and could not fully anticipate that the society in which he prospered would shortly be controlled by a political regime bent upon the annihilation of all European Jews. In that respect he reflected attitudes common among assimilated German Jews. But he was also an active member of the *Centralverein deutscher Staatsbürger jüdischen Glaubens* and noted with sorrow and frustration the degree to which many of his fellow Jews fell victim to an excessive escapism.

In his diary, usually dictated late each evening to his wife, he commented on conversations with statesmen, politicians, high military officers, civil servants, businessmen, academicians, writers, artists, Jewish leaders, and fellow journalists (especially Theodor Wolff, editor of the *Berliner Tageblatt*). Through these pages one follows the decline of the Weimar Republic as seen by an exceptionally well-informed and perceptive journalist. Feder recorded many fascinating details and impressions, but the diary does not as such contain new inside information on Weimar politics. Then as now, men in power sought to win the sympathy of important journalists without saying more than necessary. On the other hand, the diary as a whole (only selections have been published in this volume) is no doubt an excellent source for the study of liberal journalism during the Weimar Republic. The selected entries that have been published here offer fascinating glimpses into the turmoil of the newspaper world.

Unfortunately, one must note that the editorial work on this volume is not entirely satisfactory. It is understandable that the whole diary could not be published, but passages from individual entries have also been deleted without explanation. The editors include only a few explanatory annotations, a lack which is partially offset by an extensive biographical index. Scholars interested in making serious use of Feder's diary must therefore consult the original manuscript, which is housed in the Leo Baeck Institute in New York.

VERNON L. LIDTKE
*Johns Hopkins University*

JUTTA SYWOTTEK. *Mobilmachung für den totalen Krieg: Die propagandistische Vorbereitung der deutschen Bevölkerung auf den Zweiten Weltkrieg*. (Studien zur modernen Geschichte, volume 18.) Opladen: Westdeutscher Verlag. 1976. Pp. 398. DM 65.

Jutta Sywottek's *Mobilmachung für den totalen Krieg* is a curiously uneven, one-dimensional analysis of a very complex phenomenon. On the one hand, the author has succeeded quite well in analyzing a single aspect of the Nazi propaganda effort in the 1930s, namely to prepare the nation for the "heroic struggle" to come in the next war. But this was only a part of the total world view which Hitler and Goebbels transmitted in the halcyon years of the party's control. Any book on Nazi propaganda that does not build on the foundation of anti-Semitism is focusing on the periphery, not the heart of the issue.

Despite such a basic weakness, this beautifully researched book goes well beyond the standard works of Ernst Bramsted (*Goebbels and National Socialist Propaganda, 1925–1945* [1965]) and Z. A. B. Zeman (*Nazi Propaganda* [1964]) in several areas. The section on Nazi control of the newspapers and schools of journalism is masterful. The author also breaks new ground by demonstrating the importance of the theory of total war centered in the

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psychological laboratory of the Reichswehr Ministry, with links to the universities, the media, right-wing thinkers, and the Nazi Party. Readers come to see the importance of the ideas of Edgar Stern-Rubarth, Kurt Hesse, George Soldan, Albrecht Blau, Ernst Jünger, Karl Haushofer, and Erich Ludendorff in preparing the armed forces and the nation to wage total war.

Sywottek clearly analyzes the central propaganda motifs in the 1930s. Although Hitler set out to revise the Versailles settlement and prepared for an ultimate war, he went to great lengths to protest that he was an angel of peace. Each successive international crisis was presented as part of the natural order of things, whereby all German-speaking people would return "home to the Reich." The Soviet Union was accused of harboring a criminal "Jewish-Bolshevik" regime—the antithesis of National Socialist unity and justice—which starved its people and instituted a reign of terror.

The author demonstrates serious weaknesses in analyzing the Munich and Polish crises of 1938–1939. This overly detailed account, spanning over one-third of the book, makes for tedious and rather fruitless reading. Here, clearly, economy of words and precision of analysis are wanting. The problem is exacerbated by an often plodding style and the author's predilection for foreign words and phrases which further documents the lamentable decline of the German language.

One concludes from reading this work that propaganda in this period paralleled the development of Nazism itself; peace was not its milieu. It was in an uneasy phase between the "*Kampfzeit*" and World War II, when according to the Nazi world view "Idea, Sword, and Word" joined in a battle to the finish against the enemies of the swastika.

JAY W. BAIRD
*Miami University*

WALTER BAUM and EBERHARD WEICHOLD. *Der Krieg der "Achsenmächte" im Mittelmeer-Raum: Die "Strategie" der Diktatoren.* (Studien und Dokumente zur Geschichte des Zweiten Weltkrieges, Number 14.) Göttingen: Muster-Schmidt. 1973. Pp. 478. DM 90.

Among the valuable German studies available in the Operational Archives of the Naval History Division at the Washington Navy Yard are several essays in English by Vice Admiral Eberhard Weichold, including "Axis Naval Policy and Operations in the Mediterranean, 1939 to May 1943," a work some 140 typed pages in length, which he completed under contract to the U.S. Navy Department in 1951, having had full access to the captured German naval records in England. Weichold was uniquely qualified to write on that topic, for as "German Admiral in Rome" from June 1940 to March 1943, he had served as senior liaison officer to the Italian Naval Staff and, after November 1941, also had command of German naval operations in the Mediterranean. After completing his postwar studies for the Navy, Weichold set to work on a full-length account of Axis Mediterranean strategy. Though he died long before he could finish it, he bequeathed his materials to Walter Baum, who, having concluded the work as coauthor, has published it in a prestigious scholarly series as a heavily annotated historical memoir of well over four hundred pages, enhanced by photographs, maps, documents, a bibliography, and an index.

In Walter Baum the admiral found a faithful collaborator. Thus the account of the stunning British carrier aircraft attack in November 1940 on the Italian fleet harbored at Taranto, to cite but a single example, is a literal translation from the essay cited above. Generally, however, there is no way to tell precisely how much of the book originated with Baum, or to what extent he may have revised a given passage initially drafted by the senior officer of the Third Reich's least adequately supported service branch in a perilously neglected theater of the war. A veteran of that theater himself, Baum shared the admiral's convictions with intensity.

Though relatively few specialists are apt to turn to this volume for new information, few will fail to appreciate its meticulous documentation of Weichold's unwearying, but unavailing insistence on a thesis of cardinal importance in the history of the Second World War: that the entire Mediterranean basin had to be regarded as a single naval theater of operations, and that failure to wage the war accordingly could lead only to disaster.

DONALD S. DETWILER
*Southern Illinois University*

TONY SHARP. *The Wartime Alliance and the Zonal Division of Germany.* New York: Oxford University Press. 1975. Pp. ix, 220. $19.25.

When the Americans, British, and Russians created the European Advisory Commission (EAC) at the Moscow Conference in November 1943, they authorized it "to study and make joint recommendations . . . upon European questions connected with the termination of hostilities which the three governments may consider appropriate to refer to it." The questions the governments subsequently regarded as appropriate were three: the surrender instrument, the boundaries and locations of the occupation zones in Germany, and the control machinery for the occupation. The second is the subject of this revised doctoral dissertation.

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The author has set for himself two admirably precise objectives. The first is to trace in detail the steps and stages by which the zonal boundaries, including those of the French Zone and the Berlin sectors, were determined. In that by no means uncomplicated undertaking he demonstrates mastery of the British and American documentation; on the Soviet side, of course, nothing is available. His second objective is to relate the negotiations on the zone boundaries (which lasted from January 1944 to July 1945) to the concurrent military situations. In doing so he demonstrates that both the Western Allies and the Soviet Union kept a close watch on the progress of the fronts as they moved toward decisions in the EAC. The speculation on Stalin's motive for ratifying the zones protocol at Yalta is, however, at least debatable. Stalin may not have expected to be able to take more than the assigned Soviet Zone; on the other hand, he may simply have not believed that anybody, and particularly not he, would live up to the protocol if the war ended on a line other than the agreed one.

By his own reckoning Tony Sharp presents new information on more than a dozen points. The most significant is the withdrawal from the Soviet Zone after the surrender, which the British apparently not only wanted to use as a bargaining lever but for a time considered not doing at all. For the rest, what is new is mostly detail, but not frivolous detail. This is in all respects a solid and concise study in wartime diplomacy. Eight maps, incidentally, complement and clarify the text, although they omit Austria which was included as a part of the southwestern zone in the early U.S. and British proposals.

EARL F. ZIEMKE
*University of Georgia*

VOLKER OTTO. *Das Staatsverständnis des Parlamentarischen Rates: Ein Beitrag zur Entstehungsgeschichte des Grundgesetzes für die Bundesrepublik Deutschland.* (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 42.) Düsseldorf: Droste Verlag, 1971. Pp. 236. DM 48.

In contrast to the earlier studies by John Golay, Peter Merkl, Hans-Peter Schwarz, Elmer Plischke, and others, which traced the political genesis of the German Federal Republic and of its Basic Law, the work by Volker Otto attempts to analyze the ideological subsoil upon which, along with other factors, West Germany's constitution was based. The study is divided into four sections in which Otto examines: 1.) the preparatory conference at Herrenchiemsee where the main outlines of the Basic Law were conceived; 2.) the character and proceedings of the *Parlamentarischer Rat* which drafted the constitution; 3.) the chief problems encountered in connection with establishing the structural elements of the constitution; and 4.) the various conceptions of a democratic state prevailing among the council's members. The organization of the material is lucid and methodical, each section and subsection containing a summary of the main conclusions. This makes for greater clarity but also inevitably involves some overlapping and repetition. The work is based on all available German primary sources (Allied records not being particularly germane in this case), including the records of the Parliamentary Council and its committees, the archival records of the three main political parties, the protocols of the state parliaments, as well as memoirs and other writings by council members and party leaders.

Otto comes to the conclusion that the makers of the Basic Law were excessively oriented toward the past and were dominated by authoritarian (*obrigkeitsstaatlichen*) concepts. Although there was a consensus among the main party leaders that the new western rump state should be a federally organized, democratic republic (as the Allied governments had demanded in their "Frankfurt Documents" of July 1, 1948), their conception of democracy was, according to Otto, largely formalistic and legalistic, devoid of a proper appreciation of the dynamic interaction of democratic and socioeconomic processes. The men of Bonn were anxious to avoid the mistakes of the Weimar Republic (and they largely succeeded!), but they tended to attribute the demise of that regime too narrowly to constitutional, juridical flaws, Thus, the Basic Law (they refused to call it a "constitution" in order to document its provisional character) gives too much power to the executive, neglects social and economic factors, and reveals a deep mistrust of the voting masses. The men of the Parliamentary Council were more concerned with governmental stability and public order than with the realization of true democracy.

While the author carefully avoids making doctrinaire or even definitive generalizations and arrives at fairly balanced, differentiated judgments and conclusions, it seems to me that he gives too much significance to theories and concepts—with certain pejorative implications—and not enough to the hard political realities that prevailed in the late 1940s. The German Federal Republic was, after all, a child of the cold war, and its constitution reflects the sober pragmatism of its authors as well as the wishes of the Allied occupation powers (often conveyed with considerable pressure). For the whole period of the Parlamentary Council's debates the Berlin Blockade was in progress. It is characteristic for the situation of that day that it was Ernst Reuter, the plucky mayor of Berlin, who swayed the hesitant West German minister presidents to agree to the Allied proposals to form a West German state (even though West Berlin was

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not permitted to be included). He realized the necessity of consolidating the West German territories in order to strengthen the West for the common struggle against Soviet aggression.

But these are minor differences of opinion. Volker Otto's monograph is an important, informative contribution to our understanding of the ideological background of the Basic Law and will serve as a vital ingredient for a definitive study of the Parliamentary Council that I hope will be undertaken in the near future.

CARL G. ANTHON
*American University*

PETER J. KATZENSTEIN. *Disjoined Partners: Austria and Germany since 1815*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xv, 263.

This thoughtful book, which grew out of a prize-winning dissertation in political science at Harvard, is a stimulating work rather than one which answers questions comprehensively and to the full satisfaction of the reader. The author of this study seems to be aware that his examination of Autro-German relations since 1815, his theoretical abstractions, the methods of analysis, and the type of empirical documentation—in short the experiment of applying political science techniques to an eminently historical subject—will be a "source of unease" both to the social scientist for its unusual preoccupation with historical detail and to the historian "for its inexcusable predisposition for ahistorical generalization." One of Peter J. Katzenstein's concluding generalizations is that culturally homogeneous areas in general seem to have a high resistance to drives toward political unification. Regarding German Central Europe, this is surely a permissible, though hardly surprising, conclusion.

While traditional historiography has distinguished among three major phases in Austro-German relations, using 1848 and 1918 as turning points, the author, focusing especially on the problem of the persistence of Austria's political autonomy in the nineteenth and twentieth centuries, offers six distinct patterns in Austro-German relations since 1815. He calls them the aristocratic pattern (1815–1848), the conflictual (*sic*, 1848–1870), the hierarchial (1870–1918), the voluntaristic (1918–1938), the structural (1938–1945), and the pluralistic models (1945–1970). These elaborate concepts and themes, consistently adhered to and supported by numerous tables, give the treatise its character.

While providing insights, the study in parts is not easily readable. Also, some students of Austrian history will entertain doubts about the adequacy of the foregoing terms and concepts and the new periodization; others will question one or the other generalization or interpretation. Hitler's main interest regarding Austria allegedly consisted in exercising "political control" rather than in aiming ultimately at amalgamation (p. 152). Katzenstein's description of the Austrian experience between 1938 and 1945, not just the perplexing first months after the annexation, as "conquest by consent" is likewise debatable. In an appendix he finally suggests that his case study of Austro-German relations might serve as a pilot study for a broader comparative inquiry on the relationship between culturally cohesive states. Even historians who are not unsympathetic to the comparative approach will have to examine carefully the results of such efforts.

ALFRED D. LOW
*Marquette University*

*Mitteilungen des österreichischen Staatsarchivs*. Volume 28, *Festschrift: Walter Goldinger*. Vienna: Ferdinand Berger and Sons. 1975. Pp. 556.

This volume renders appropriate honor to Walter Goldinger, who is retiring as director of the *Haus-, Hof- und Staatsarchiv*. Goldinger during his career has contributed 123 widely diversified publications to historical and archival studies, including particularly his *Der geschichtliche Ablauf der Ereignisse in Österreich, 1918–1945* (1954). He carried on the tradition of Alfred von Arneth, Árpád von Károly, Oskar Mitis, and, in more recent years, Gebhard Rath, Richard Blaas, and others of closely combining the functions of the historian and the archivist. This volume is notable testimony to that tradition.

The principal categories of the studies are archival planning and techniques, special sections of several Austrian archives, archival management, significant archivists, and archival history. In the first group Leopold Auer's article on electronic data-processing shows much sensitivity to developments in various parts of the world, while Otto F. Winter's study of the *Kriegsarchiv* demonstrates the archival problems resulting from the *Anschluss*. Bernard Zittel, Paul Mechtler, and Wilhelm Rausch have made similarly valuable contributions. Erich Hillbrand discusses the map collections in the *Kriegsarchiv* in the next category, where the *Nachlass* of Deffonseca, material on Austro-Siamese relations, the *Militärkanzlei* of Francis Ferdinand, and the *Nachlass* of Conrad are also treated by Anna Coreth, Anna Benna, Rainer Egger, and Peter Broucek. Franz Dirnberger deals with theater history from an archival point of view. Franz Kürschner, Ferdinand Krackowizer, and Hermann Hango are the archivists specifically studied. Among the articles on archival history, Richard Blaas's treatment of the handling of Italian

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material after 1866 and Rudolf Neck's study of Oswald Redlich's work are especially significant. In general, this volume is a valuable *Festschrift* by the very people with whom the scholar working in Austria deals.

MARVIN L. BROWN, JR.
*North Carolina State University*

GRETE KLINGENSTEIN. *Der Aufstieg des Hauses Kaunitz: Studien zur Herkunft und Bildung des Staatskanzlers Wenzel Anton.* (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, number 12.) Göttingen: Vandenhoeck and Ruprecht. 1975. Pp. 327. DM 62.

Having begun her research with the intention of writing a biography of the most famous of the Kaunitzes, Maria Theresa's chief minister, Grete Klingenstein first narrowed her focus to the origins and youth of Wenzel Anton (1711–1794), then redirected it to the family as a case study of the rise of a Moravian house in the Austrian service nobility. This volume retains the marks of this shifting focus.

It opens with a thorough and judicious review of the literature on Prince Kaunitz. Part one recounts the progress of the house, principally through the careers of Dominik Andreas (1655–1705) and Maximilian Ulrich (1679–1746). Klingenstein intelligently blends painstaking basic research, including a thorough study of the family archives in Brno, and contemporary analyses of social history, in particular drawing on the insights of Otto Brunner. Part two presents in detail the results of her efforts to track the elusive Wenzel Anton. Most impressive is the scope of her research which results in an evocative description of the intellectual and cultural environment of Leipzig during his student days there (1730–1732), and of the form and substance of the traditional cavalier's tour. She also details Kaunitz's entry into public service, closing with an epilogue that describes his assumption of the state chancellorship.

The very thoroughness of the research emphasizes the biographical problem and explains the shifting focus. The paucity of materials which directly document the development of Kaunitz forces the historian to reconstruct painstakingly the context in which he lived and moved, surrounding him, but never quite capturing the persona. For that reason we may never have a biography of Kaunitz, properly speaking, but Klingenstein's work, if continued at this level, will be just compensation. While this volume seems less a book than a collection of research reports on topics impinging upon one or another of her foci, the breadth and depth of the research and the interpretive skills here demonstrated make this an essential work for students of Habsburg history in the seventeenth and eighteenth centuries.

WILLIAM J. MCGILL
*Washington and Jefferson College*

ADAM WANDRUSZKA and PETER URBANITSCH, editors. *Die Habsburgermonarchie, 1848–1918.* Volume 2, *Verwaltung und Rechtswesen.* Vienna: Verlag der Österreichischen Akademie der Wissenschaften. 1975. Pp. xviii, 791. DM 120.

Published under the supervision of the Austrian Commission for Habsburg History, this volume contains ten chapters, written by well-known experts in Habsburg affairs. The focus of the volume is on the workings of the administration and the judicial system of the monarchy between 1849 and 1918. If one may speak of a thesis it is that the variety of administrative techniques used in the lands of the monarchy were often opportunistic and uncoordinated, and that their failure was rooted in the contradictory aims of the bureaucracy. On the one hand, the efficient operation of the system was highly desired; on the other, a high level of efficiency could not be attained because the multinational character of the monarchy required almost superhuman competence by bureaucrats and judicial personnel.

Although it is difficult to single out any of the competent historians contributing to this volume, one must mention the chapters written by Bárány, Novotny, Sarlós, and Kann. Bárány sees a direct relationship between modernization and administrative reform. According to him, the Habsburg administration in Hungary failed to perceive this direct relationship, hence its failure. Novotny's discussion of the role of royal councillors throws light on a much-neglected subject. Sarlós, focusing on the judicial system in Hungary, maintains that this system was too backward and too heavily weighted against the lower classes and the nationalities. Finally, Kann examines the contradictions between the administrative and judicial reforms and the multinational character of the monarchy, concluding that these contradictions were irreconcilable. He maintains that while administrative efficiency would have required increasing centralization of bureaucratic functions, national and ethnic differences made this impossible.

In retrospect, one is almost compelled to agree with the editor, Adam Wandruszka, that inefficient as the Habsburg administration and judicial system were, they were not as bad as what came after them. The Balkanization of the East European bureaucracies after the collapse of the monarchy came about not because of the failure of the Habsburg system, but because of its destruction by the successor states. This volume should

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help to set the record of that much-maligned administration straight.

JOSEPH HELD
*Rutgers University, New Brunswick*

GUNTHER E. ROTHENBERG. *The Army of Francis Joseph*. West Lafayette, Ind.: Purdue University Press. 1976. Pp. xiii, 298. $12.00.

Francis Joseph had tears in his eyes when he signed the war manifesto against Serbia in 1914, and Conrad was pessimistic about the chances of victory. Save for significant Czech defections, however, the army fought bravely and resisted disintegration until the last months of World War I. Gunther E. Rothenberg demonstrates in this fine study the validity of Oscar Jászi's conclusion that the military arm was one of the empire's strongest centripetal forces, despite the many afflictions it had to endure.

The emperor deserved much of the blame for unprogressive attitudes and poor leadership. He was suspicious of bright officers and unable to comprehend or tolerate many of the developments in the art of war that radicalized the conduct of hostilities. From 1849 to 1906 he countenanced the reformist tendencies of only one man, Baron Franz Kuhn, the Minister of War, from 1868 to 74. Field Marshal Friedrich Beck had introduced repeating rifles, machine guns, and quick-firing artillery by 1905, but stagnation was the word for his last ten years. Conrad was a distinct improvement over Gyulai, the incompetent of 1859, but Francis Joseph's reliance on utterly loyal and unquestioning general officers disadvantaged the competence and resilience of the increasingly bourgeois majors, captains, and lieutenants.

The Habsburg armed forces received less money than those of any other European power, and the conscripts were often illiterate. Failure to keep up with technology was not entirely a matter of the ruler's obtuseness and his advisers' conservatism. Rothenberg believes the army escaped the nationalistic, religious, and economic conflicts that beset the monarchy, but he also insists that such relative neutrality isolated the soldiery from the people. German remained the basic medium of communication, save for some concessions the Magyars secured after the struggle of 1902–06.

Rothenberg is fair in appraising the military leaders, drawing upon archival material to offset some of the inevitable hagiography of published sources. He illuminates the generals' tendency to plot "preventive wars" against Italy and Serbia while neglecting preparations against Russia. He also makes clear the lack of rapport between Vienna and Berlin in planning joint operations between 1879 and 1914. This is a judicious and well-expressed addition to the growing literature on Francis Joseph's era.

WILLIAM A. JENKS
*Washington and Lee University*

ROBERT A. KANN. *Erzherzog Franz Ferdinand Studien*. (Veröffentlichungen des Österreichischen Ost- und Südosteuropa-Instituts, volume 10.) Vienna: Verlag für Geschichte und Politik. 1976. Pp. 256.

Only a small minority of historians ever receive a *Festschrift;* very few get two. This work, a collection of essays by Robert Kann on Franz Ferdinand, half new and half previously published but revised for this volume, is in effect a second *Festschrift* published by Austrian colleagues in honor of Kann's seventieth birthday.

Without doubt Kann deserves the honor for his services to Austrian history on both sides of the Atlantic. But does Franz Ferdinand deserve more attention? The author faces the question squarely. Not, he grants, for the archduke's character and personality, which are mainly repulsive; not for his "reform" ideas, which were intrinsically unsound; not even for his influence, which was important but never decisive; and certainly not for the sake of speculating on what he might have done in power. One thing, however, draws our interest to Franz Ferdinand. He represents a final, pure, somehow impressive flowering of the monarchic spirit and personality in its downfall, an ideal type of the man who would be ruler and not leader. Fair enough. But one suspects, all the same, that there is not enough material here to justify so many closely related essays. Much of the material presented consists of excerpts from the archduke's correspondence with his advisers, often only loosely connected with the theme of a particular essay. Inevitably there is some repetition both in substance and exposition. In the longest essay, Kann expands and deepens, but does not alter, his earlier devastating indictment of Count Czernin, and Kann's (to me) most important contribution in showing Franz Ferdinand as head of the pre-1914 peace party has appeared before in a different form.

Nonetheless, there is ample evidence here of what makes Kann an outstanding historian—a thorough mastery of the archives and literature, intimate acquaintance with the personalities and milieu involved, and penetrating critical judgment. Though not one of Kann's major works, this is a very useful book on Franz Ferdinand and his advisers, and a richly deserved honor to its author.

PAUL W. SCHROEDER
*University of Illinois, Urbana-Champaign*

ANDREW G. WHITESIDE. *The Socialism of Fools: Georg Ritter von Schönerer and Austrian Pan-Germanism.*

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Berkeley and Los Angeles: University of California Press. 1975. Pp. x, 404.

Adolf Hitler resurrected Georg von Schönerer in the early pages of *Mein Kampf*, and most students of Pan-Germanism and National Socialism have ploughed through the biased accounts of him that appeared in the Third Reich. The present study should discourage future biographers and political analysts, for it is exhaustive in scope and shrewd in judgment.

The chief exponent of Austrian *Alldeutschtum* had a vocational education, took over the management of his father's estate at twenty-six, and never lacked for money. As a youth he was rude, stubborn, and immune to religious sentiment. Elected to parliament in 1873, he was at first a conventional agrarian radical, much concerned with the plight of the peasantry. In a few years he was attacking the entire Austrian establishment, though "he never showed signs of having a firsthand acquaintance with any original thinker or writer" (p. 75). At the height of his notoriety he was against liberals, Jews, Slavs, Habsburgs, and, after 1900, the Roman Catholic Church. Accused of assaulting the editorial staff of the *Neues Wiener Tagblatt* in 1888, he was surprisingly hypocritical, even fawning, in his address to the court. After serving a term in prison, he recaptured much of his support in the campaign to nullify Badeni's language ordinances of 1897. Despite a good showing in the elections of 1901 by his faction, he was something of a fallen idol by 1905.

Andrew G. Whiteside expertly illuminates these rather familiar developments. In the process he gives as good a description of the constantly fluctuating Austro-German parties as can be found. It is no surprise that the liberals feared Schönerer's ability to stir up the masses. An outstanding chapter deals with the *Los von Rom* movement and the relationship of the German Empire's Pan-German League with the Austrian phenomenon. Lutheran pastors soon realized that the Austrian Pan-German leaders had no religion, for they admitted frankly that their conversions were for political purposes. The rank and file who deserted Roman Catholicism usually were from the ethnic borderlands and the middle class, rather than peasants and workers. Schönerer aged rapidly and became too lethargic to proselytize the students as he had in the 1880s. The Pan-Germans of the Hohenzollern state were friends of the alliance with Austria-Hungary and ill-disposed to welcome its dissolution, as preached by Schönerer. Cooperation with the anti-Semites of William II's empire was just as disappointing.

Schönerer's own vanity, suspiciousness, and quarrelsomeness would have fragmented any party. The tough workers of the besieged German enclaves in Bohemia and Silesia did not see him after 1901, nor did he bother to scare the bourgeoisie with evocations of Marxian violence. There was good reason for Hitler to conclude that the movement "lost the future."

WILLIAM A. JENKS
*Washington and Lee University*

HELENE MAIMANN. *Politik im Wartesaal: Österreichische Exilpolitik in Grossbritannien, 1938–1945*. (Veröffentlichungen der Kommission für Neuere Geschichte Österreichs, volume 62.) Vienna: Hermann Böhlaus Nachf. 1975. Pp. xv, 355. DM 74.

The handicaps that Austrian exiles in Britain faced between 1938 and 1945 were as much psychological and intellectual as they were material. Added to their myriad woes was the fundamental dilemma of what it meant to be Austrian, and what Austria itself represented. The Social Democrats, wedded to Otto Bauer's doctrinaire views, flatly rejected the concept of an independent Austrian nation. Marxist rigidity tied him and the entire Austrian Socialist leadership in exile to the idea of a *grossdeutsch* social revolution coming at the end of an imperialist war. In contrast to the Socialists, the Austrian Communists were able to champion ideas and goals that were both popular among the exiles and well within the realm of possibility. The Communists were enthusiastic supporters of the concept of an Austrian state fully sovereign and independent of any ties to Germany. Their positive stance toward Austrian nationhood had developed in the heyday of the Popular Front and largely mirrored the needs of a Soviet-dominated Comintern; yet, as Helene Maimann points out, a few individual Communists vigorously pushed forward the notion of an independent Austrian state, Alfred Klahr being the most important of these thinkers.

While the Communists were able to dominate the Free Austrian Movement that claimed to speak for exiled Austrians in wartime Britain, theirs was largely a hollow victory. Communist primacy in the FAM mattered little in the larger political picture, since the organization never received official recognition from the British government. The Moscow Declaration of 1943 on reconstituting an Austrian state made the professional politicians among the exiles look ahead to their postwar careers. While many of the exiles never returned to Austria, most of those who came home did so with new ideas and attitudes derived from the difficult exile years. Above all else, the Socialists abandoned their ideological rigidity. As Kurt Shell has pointed out, the post-1945 Austrian Socialists drew upon the experience of the Labour party much more than the pronouncements of Marx. The Communists, after their half decade of popularity and influence in Britain, returned to Austria to the rude shock of repudiation at the polls.

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Maimann's study of the intricacies of exile politics still bears the marks of a doctoral dissertation. While it is conscientiously researched, it too often falls back on narration where probing analysis would have illuminated basic issues. In sum, however, this is a creditable study of ideologically charged politics taking place in a power vacuum.

JOHN HAAG
*University of Georgia*

CHRISTIAN PFISTER. *Agrarkonjunktur und Witterungsverlauf im westlichen Schweizer Mittelland zur Zeit der Ökonomischen Patrioten, 1755–1797: Ein Beitrag zur Umwelt- und Wirtschaftsgeschichte des 18. Jahrhunderts*. Bern: Lang Druck. 1975. Pp. 229.

The author of this exhaustively researched monograph has posed the following question as the basis for his study: "How were food production and agrarian legislation in the western Swiss plateau affected by meteorological conditions during the four decades 1755–1797?" (p. 202). Not surprisingly, he finds positive correlations between bad weather, smaller harvests, and higher agricultural prices; equally unastounding is his conclusion that people are often stimulated by crises in the means of subsistence to investigate and do something about the causes and effects of inadequate food supplies. Yet these and some other rather simplistic conclusions hardly describe the contributions of the book. At one level, Christian Pfister's work can be seen as a narrowly focused and highly detailed and sophisticated examination of the relationship between climate and the availability and market price of various agricultural products in a relatively small area of western Switzerland, and as a commentary on some specific human responses to alterations in the food supply through inquiry, technical innovation, and public policy in the same area. Among the human responses that the author discusses, one is of special importance to his own research: the founding in 1759 of the Economic Society of Berne, which established the first primitive network of standardized meteorological instruments in western Switzerland and provided him with a wealth of recorded data probably unequalled in any area of Europe at the time. Indeed, the whole book is a statistical *tour de force*, and as such is testimony to the author's diligence and technical competence; it is also likely, however, that excessive detail will deny it the wider audience to which a number of its results and observations should otherwise entitle it.

That wider audience, however, might well benefit from the wealth of general information on the various influences of different meteorological phenomena on crop yields, on the numerous variables which affected price fluctuations of agricultural products differentially and some fascinating observations on weather in general. The latter include climatological comparisons between the late eighteenth and early nineteenth centuries and the twentieth century, which tend to confirm others' suggestions that ours has been a century of unusually favorable meteorological conditions. Such observations, scattered throughout the book, are likely to interest the professional readership more than Pfister's "attempt at a model of ecologic crisis" (p. 193), whose simplicity will do little to inform anyone even vaguely familiar with the economic or agrarian history of preindustrial Europe.

JOHN G. GAGLIARDO
*Boston University*

CARLO M. CIPOLLA. *Public Health and the Medical Profession in the Renaissance*. New York: Cambridge University Press. 1976. Pp. viii, 136. $14.50.

Three years ago Carlo Cipolla, an economic historian, provided us with a detailed and valuable study of how one man, a public health officer, fought the plague in a small Italian town in the 1630s. Apparently, Cipolla's careful cultivation of the rich archives of northern Italy yielded much more than could be harvested in *Cristofano and the Plague*, for this more recent work is further fruit from that same vineyard.

The latest harvest is, in fact, twofold. In the first section, Cipolla provides precise details on the origins of the Health Boards in the major cities of Venice, Florence, and Milan, stimulated by the Black Death and subsequent waves of plague epidemics in the fourteenth and fifteenth centuries. Emphasis on the early appearance and increasing sophistication of organized public health in this region is a welcome corrective to our frequently too narrow focus on later developments in England, developments which, Cipolla says, owed much to their Italian precursors.

The second section concentrates on the characteristics of the medical profession in Tuscany in the early seventeenth century. Through bureaucratic records, kept by the public health boards during the plague of 1630, Cipolla has developed a detailed statistical analysis of the physicians and surgeons practicing in the Florence, Pisa, and Siena areas, their numbers, ages, educational background, place of birth, marital status, and geographical distribution. Most striking is the relatively plentiful supply of physicians and surgeons in rural as well as urban areas.

While some consideration is given to the relationship of the health boards to the medical profession, the plague itself provides the chief underlying theme for the book. Plague epidemics moved men to establish institutions and to keep records, and, we might add, the movements of men in turn

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fostered plague epidemics. *Public Health and the Medical Profession in the Renaissance* is a significant contribution to the growing body of literature by which social, economic, and demographic historians are uncovering that dialectic between natural and social history which produced the modern world.

DONALD G. BATES
*McGill University*

PETER E. BONDANELLA. *Francesco Guicciardini.* (Twayne's World Authors Series, number 389.) Boston: Twayne Publishers. 1976. Pp. 160. $7.50.

This book is a brief, routine intellectual biography of the Florentine *homo politicus* and matchless Renaissance historian. But there is less reason than usual to complain about its quite ordinary purposes: "a detailed account of Guicciardini's major contributions to Renaissance thought . . . of use not only to the general reader but also to Renaissance specialists" (p. 9). Guicciardini's power and weight, aristocratic airs, corrosive but confining brilliance, and pose of self-sufficiency have always been forbidding. In Italy he has been a "case" to test moral, methodological, or patriotic commitments, even though (and perhaps partly because) he has been known piecemeal, since a number of his works have been published in scattered editions only during the past century. Despite new interest, which has produced English translations of his works, Guicciardini remains dispersed among monographs, articles, or chapters of books on Florentine history and Renaissance political thought, historiography, or literature. He is still, in ways that would have pleased him, difficult to manage and impossible to miss.

Peter E. Bondanella sorts out and synthesizes the contemporary scholarly consensus, from Roberto Ridolfi's philologically precise and personally expressive biography to the analyses of Guicciardini's political and historical works by Vittorio de Caprariis, Rudolf von Albertini, Felix Gilbert, Nicolai Rubinstein, and Emanuella Scarano. These studies serve as a gridwork for his own reading of Guicciardini. After a biographical chapter, Bondanella follows Guicciardini, and the best scholarship, through the class-conscious, family-oriented early *History of Florence* (chapter 2), political and autobiographical works (chapters 3–6), and the purportedly humanistic *and* empirical *Florentine Affairs* (chapter 7), to the culminating *History of Italy* (chapter 8). Chapter 9 concludes with an "assessment" of Guicciardini's "place in world literature," which is actually a discussion of his reputation and the diffusion of his works. An annotated bibliography is appended.

The traits Bondanella delineates in Guicciardini will seem, at least to the *cognoscenti*, as familiar and insistent as the features in his portrait by Bugiardini. The ideological prejudices of the Florentine aristocrat, the self-interest in practice and principle, the critical empiricism and historical sense, and the driving, and in the end saving, preoccupation with method come forward in full relief. The pattern of development is by now the standard one of movement from more parochial, more political commitments to a comprehensive historical vision, exacting and resigned, against a background of personal misfortune and the loss of both Florentine and Italian independence. Even elusive points—the problematic relationship between the public figure and the private man, or a specificity of thought which one talks about without exactly conveying—are predictable. There is nothing new in this; perhaps in a man as "once-born" as Francesco Guicciardini there never could be. What counts is that Bondanella's book is intelligently clear, comprehensive, and generally reliable. It should do much to make Guicciardini and his historians more accessible.

RANDOLPH STARN
*University of California, Berkeley*

ALBANO BIONDI *et al. Eresia e Riforma nell'Italia del cinquecento: Miscellanea I.* (Biblioteca del "Corpus Reformatorum Italicorum.") DeKalb: Northern Illinois University Press, and Chicago: The Newberry Library. 1974. Pp. 356.

PAOLO NARDI. *Mariano Sozzini: Giureconsulto senese del quattrocento.* (Quaderni di "Studi Senesi," number 32.) Milan: Dott. A. Giuffrè Editore. 1974. Pp. xiv, 203.

The Biblioteca del Corpus Reformatorum Italicorum, under the direction of Luigi Firpo and Giorgio Spini with the collaboration of Antonio Rotondò and John A. Tedeschi, has undertaken the important task of publishing hitherto unavailable sources of the heretical and reformation movements of sixteenth-century Italy. This volume is a collection of monographic essays which illuminate many of the most important problems in the religious history of the period. Within the scope of this review it is impossible to discuss in detail the significance of these contributions, but a brief summary of the contents gives an impression of the variety and interest of the subjects treated.

In the first essay Albano Biondi analyzes the theme of simulation and dissimulation and points out that it extends far beyond the Nicodemism condemned by Calvin to include attitudes toward social behavior recommended by authors as diverse as Castiglione, della Casa, Guicciardini, and Montaigne. He suggestively concludes that "on the road which leads from Nicodemism one meets also Tartuffe."

Sylvana Seidel-Menchi discusses the reactions to

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Erasmus by two lesser-known figures, Bartolomeo Cerretani in Florence and Battista Casali of the Roman Academy. She also analyzes the influence of Erasmus on one of the famous martyrs of the Italian Reformation, Aonio Paleario, whose religious orientation seems, on the basis of a letter of Paleario to Erasmus of 1534 (wrongly dated 1536 on p. 130), to have owed much to the earlier ideas of Erasmus.

Carlo Ginzburg and Adriano Prosperi reconstruct the successive versions of the much discussed *Beneficio di Cristo*, and interpret its significance in the light of the debates, around the middle of the century, on predestination and free will, particularly as these themes were formulated in certain centers of the Benedictine Order. Aldo Stella describes the ecclesiological doctrines of the group of Anabaptists who were condemned at Trieste in 1540. Carlo Ossola analyzes the previously unknown Italian translation and commentary on St. Matthew by Juan de Valdés and its significance for his religious thought in the last period of his life. Uwe Plath writes on the objections of the authorities in Basel to the publication of the *De amplitudine beati regni Dei* of Celio Secondo Curione in 1554.

The economic activity and ideas of an Italian exile are the subject of the contribution of Manfred Welti. He describes the extraordinary career of Giovanni Bernardino Bonifacio, a wealthy Neapolitan, who fled for reasons of conscience and invested his capital with northern bankers but who later renounced the taking of interest and lost his fortune.

The final essay, by Luigi Firpo, gives the list of capital executions in Rome from 1567 to 1671, drawn from the dispersed archives of the Arciconfraternità di San Giovanni Decollato, whose members had the duty of comforting the condemned in their last moments.

Sources made available in this volume include two letters to Erasmus, one from Casali, and one from Aonio Paleario, published by Seidel-Menchi; six letters from the Mantuan Archive from the correspondence of Ercole Gonzaga, concerning the monks of San Benedetto, published by Ginzburg and Prosperi; three letters on the Curione affair published by Plath; and the two inventories in the Firpo article of those condemned to capital punishment.

The high level of scholarship in these essays and the new data and new interpretations which they include make this publication one of great importance for all students of the Reform in Italy.

Paolo Nardi's scholarly study of the quattrocento Sienese jurist, Mariano Sozzini, appears in the series *Studi Senesi*, under the sponsorship of Domenico Maffei, the distinguished historian of law at the University of Siena.

The first chapters trace the biography of Sozzini, his early education in Siena, his training in canon law under Niccolò dei Tedeschi, the great Panormitanus, his eventual appointment to succeed his master, and with the growth of his reputation, his opportunities to teach at Florence and Ferrara.

As a highly successful jurist he was closely allied with many leading figures of his generation, foremost among whom was Aeneas Sylvius Piccolomini, Pope Pius II, who had been a fellow student at Siena and with whom he always maintained a close friendship. Although Aeneas Sylvius somewhat ironically praised Sozzini as an "*uomo universale*," he had a sincere regard for him and, as Pope, appointed him a consistorial advocate.

In the analysis of Sozzini's legal works and his method of interpreting the law, Nardi revises the older opinion which had exaggerated the extent to which Sozzini was influenced by humanism. In fact it appears that, although he had a considerable acquaintance with classical authors, he never used this knowledge as an instrument for reconstructing the history of institutions. Nardi concludes that, "In the very years in which the humanist philology, known but not applied by Sozzini, was becoming the instrument which his descendants would use to question the foundations of traditional theology, and the jurists of the *mos gallicus* to discredit the methodology of the school of commentators, the work of Mariano il Vecchio remained that of an authoritative exponent of the older juridical tradition which was leading to the school of the *mos italicus*."

Appendix I contains thirty-five pages of documents illustrating Sozzini's life and work, and Appendix II a bibliography of all manuscripts of his works and all editions published in the fifteenth, sixteenth, and seventeenth centuries.

MYRON P. GILMORE
*Harvard University*

RICHARD TILDEN RAPP. *Industry and Economic Decline in Seventeenth-Century Venice*. (Harvard Historical Monographs, number 69.) Cambridge, Mass.: Harvard University Press. 1976. Pp. ix, 195. $16.50.

J. R. HALE, editor. *Renaissance Venice*. Reprint. London: Faber and Faber; distributed by Rowman and Littlefield. 1976. Pp. 483. Cloth $35.00, paper $12.50.

Although the sixteen articles in *Renaissance Venice* do not share a single theme, two concerns predominate: the crisis created by the League of Cambrai and the heightened interest in the *terra ferma* in the fifteenth and sixteenth centuries coupled with the increased instability of maritime trade. Stanley Chojnacki dispels some old misconceptions by revealing that the composition and internal configuration of the Venetian patriciate

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changed considerably in the fourteenth century. David Herlihy uses Verona as a case study of population growth in the Veneto during the fifteenth century. Michael Mallett tracing the maturation of the permanent Venetian army, Frederic C. Lane describing fleet organization, Donald E. Queller showing the development of ambassadorial *relazioni*, Nicolai Rubinstein discussing Italian reactions to *terra ferma* expansion, and Alberto Tenenti analyzing the psychological implications of an altered sense of space and time are concerned with the changing Venetian *imperio* of the fifteenth and early sixteenth centuries. Felix Gilbert's essay, "Venice in the Crisis of the League of Cambrai," provides a focal point for the collection, for the other articles touching on sixteenth-century issues also tend to see Cambrai as a watershed. In the outstanding contribution to the collection, Gaetano Gozzi's study of legal institutions in the post-Cambrai period reveals an increased concentration of power in the hands of a small minority of the patriciate and a tendency of the government to rely on authoritarian principles in place of equalitarian traditions. Ugo Tucci and Brian Pullan document the progressive detachment of the nobility from trade in favor of more stable investments such as mainland properties. Three articles are primarily intellectual history: Vittore Branca writes about Ermolao Barbaro and the characteristics of Venetian humanism at its apogee, Myron Gilmore reviews the sources for the "myth" of Venice, and William Bouwsma discusses the contribution of the Venetian tradition to the political discourse of Europe. Distinctly different in content and purpose are Paolo Prodi's suggestions for research on the structure and organization of the Church in Venice and Terisio Pignatti's study of Dürer's contacts with Venetian artists. Most historians, even specialists, will use the collection selectively, an approach made difficult by the high price of even the paper edition.

Richard Rapp's careful little book does not offer any revolutionary reinterpretations of seventeenth-century Venetian economic history but clarifies the problem of the "decline" of Venice from commercial hegemony to relative insignificance and provides a useful case study of an early modern economy that never "developed." Rapp's thesis hinges on a distinction between relative decline—Venice lost her commercial supremacy—and absolute decline, which did not occur in seventeenth-century Venice because it retained the capacity to support the population at a certain standard of living. He argues that the Venetians maintained domestic prosperity through structural adjustments in guild employment and technological innovation but lost their export market through conservative government policies.

Rapp's fine book is a must for Venetian specialists; the clarity of his prose and the straightforward exposition of economic theory and research techniques should make it unusually useful for courses in economic history.

EDWARD MUIR
*Stockton State College*

R. A. WEBSTER. *Industrial Imperialism in Italy, 1908–1915*. Berkeley and Los Angeles: University of California Press. 1975. Pp. vii, 392. $22.75.

The shift in Italy's foreign relations which led to estrangement from its Triple Alliance partners and ultimately to entry into World War I on the side of the Entente powers has conventionally been interpreted as a natural realignment reflecting historic antagonism between Italy and Austria-Hungary. Although R. A. Webster acknowledges this rivalry and concern for diplomatic and military balance, he argues that Italian foreign policy changed direction primarily in response to the nation's economic needs. The origins of Italian heavy industry owed much to government subsidies inspired by a desire to attain great power status. Once created, these enterprises were constrained, by weak private demand, capital shortages, and for a time by technological inadequacies, to press the state for continued support. Paradoxically, the boom years of the early Giolittian period strengthened this interdependence by expanding industrial capacity far beyond what could be absorbed by the domestic market.

An imperialist policy to exploit potential markets in the Balkans and Anatolia appeared as a dynamic solution to the problems of Italy's marginal industrial development. Germany and Austria-Hungary resented Italian penetration into their "work zone." At the same time, Italian industrialists, equally resentful of German attempts at financial and technical control, turned to the Entente powers for raw materials and capital on relatively easy terms.

Apart from the well-documented and persuasive elaboration of this thesis, perhaps the major specific merit of this work lies in its examination of the records of the Banca Commerciale of Milan (closely linked to the Giolittian political establishment) and the correspondence of its chief officers. Webster demonstrates that the great bank, if not quite the dominant element alleged by opposition groups, did participate significantly in government economic decisions and foreign policy.

The excellent scholarship notwithstanding, one must still enter a few caveats. If Italian and German imperialism stemmed from structural imbalances produced by late, rapid industrialization, how does one explain British and French imperial leadership? Again, to suggest a "convergence" (p. 190) between nationalists and industrialists is

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somewhat misleading. Coincidence of views might better describe their common interest in overseas expansion since nationalist ideologues often vehemently rejected the bourgeois values represented by these industrial interests. Finally, one should consider Vilfredo Pareto's analysis of government economic policy with caution. Pareto's passionate commitment to free trade led to a forty-year vendetta that warped his perspective.

Unfortunately there is no separate bibliography, but the footnotes in the back provide full bibliographic citations and much other useful information. One could wish for maps to illustrate Italian economic interests in Anatolia and the Balkans, but their absence is not critical. The book contains an excellent index.

Webster has given us a major work on a major subject. It should be read not only by specialists in modern Italian history but also by those interested in prewar diplomacy and Ottoman decline. Even those who question his conclusions will probably admit that the author has presented his case cogently and persuasively.

ARMAND PATRUCCO
*Rhode Island College*

EUGEN STANESCU, editor. *Studii istorice sud-est europene*. Volume 1. Bucharest: Editura Academiei Republicii Socialiste România. 1974. Pp. 212. 19.50 L.

With these four essays, the Institute for the Study of Southeastern Europe at Bucharest inaugurates a series of occasional volumes containing the research of its members. The prospectus also indicates that these studies will be of a multidisciplinary character. In this instance the articles deal with Romanian history as recorded in Serbian chronicles of the fifteenth to seventeenth centuries, the Ottoman conquest of Dobruja, Russo-Serbian relations (1806–1812), and the culture and administration of the "Greek" company of Braşov. Exhaustively researched and footnoted, each article includes at least a cursory survey of the pertinent historiography. The first three seldom explore new material and display a rather leaden style in their detailed chronology of largely familiar events. Cornelia Papacostea-Danielopolu's essay on the "Greek" commercial community of Braşov, however, reflects a thorough investigation of the archives in Braşov and the Academy Library. Her analysis of the culture and structure of this group underlines the legal status it enjoyed within the monarchy. Furthermore, the multidisciplinary intent of the series is apparent not only in her discussion of the church and schools, but also in the development of Romanian motivations for participation and the impact of the traditional Macedonian community exporting its merchants to Transylvania.

WILLIAM O. OLDSON
*Florida State University*

SPYRIDONOS G. FOKAS. *Hoi Hellenes eis ten Potamoploian toy kato Doynabeos* [The Greeks in Riverboating on the Lower Danube.] Translated by MARIA I. MARKOPOULOY. (Institute for Balkan Studies, Serial number 144.) Thessaloniki: Institute for Balkan Studies. 1975. Pp. x, 478.

Long after the Ottoman Empire receded from the Balkan peninsula and independent nation-states emerged, its legacy of ethnically diverse communal life continued to survive in many areas. Spyridonos G. Fokas' study deals with one of those communal worlds, that of the Greeks who settled along the Danube in the Romanian principalities.

In Greece there has been a surge of interest recently regarding the life of the Greek diaspora throughout the Balkans and the Near East. The title, *The Greeks in Riverboating on the Lower Danube*, accurately reflects the contents, which deal with the rather narrow theme of Greek navigation and shipping along that waterway. This testifies to the author's long and personal acquaintance with his subject matter; his family lived in the Danubian port cities of Sulina and Brăila, and the author himself served as secretary to the board which administered the naval affairs of the Danube.

Ever restless and seeking new trading areas, the Greeks settled along the shores of the Black Sea centuries ago. Gradually they moved into the Danube delta following the course of the river into the interior. By the time the great empires arose in the area and competed for influence there, the Greek merchant communities were well established. Fokas with a touch of national pride chronicles the fate of the Greek shippers as they sought to counter the increasing political and economic pressures they faced during the nineteenth and twentieth centuries. He makes abundantly clear that these people prospered most in the days before the First World War when a few great states regulated the commerce of the river. Once the eastern empires collapsed and aggressive nationalist states replaced them the days of economic freedom for foreign communities were doomed.

Fokas dwells upon political developments, especially international treaties, and technical problems, such as types of ships and methods of transport. Consequently, the life that the Greeks experienced emerges only in an oblique manner. Fully one-fourth of the work is made up of tables and appendixes while the text itself is awash with statistics, but they do little more than provide a compendium of shipping and a catalog of tonnage. A fine selection of photographs at the end provides

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us with a glimpse into that once thriving social and economic world. Unfortunately the text does not turn that glimpse into a full vision.

GERASIMOS AUGUSTINOS
*University of South Carolina*

STANLEY B. WINTERS and JOSEPH HELD, in collaboration with ISTVÁN DEÁK and ADAM WANDRUSZKA. *Intellectual and Social Developments in the Habsburg Empire from Maria Theresa to World War I.* (East European Monographs, number 11.) Boulder, Col.: East European Quarterly; distributed by Columbia University Press, New York. 1975. Pp. 304. $14.00.

I enjoyed reviewing these valuable collectanea written by friends and colleagues on both sides of the Atlantic and dedicated to one of my own mentors. This *Festschrift* stays well under the circus-tent-sized umbrella of "intellectual and social developments in the Habsburg Empire from Maria Theresa to World War I" and is a most welcome relief from purely political and economic studies. To my satisfaction many authors also picked up the theme of nationalism.

Considering the muster of Habsburg specialists, the scholarship is, unsurprisingly, impeccable. Each contribution is carefully handcrafted and meticulously documented. As regards *Heuristik*, *Kritik*, and *Auffassung*, the book is an unqualified success. In spite of the importance and interest of the several topics, however, the *Darstellung* is weak. With rare exceptions the paragraphs plod on in leaden style. (Will Americans and Germans ever learn to write with the style and readability of our British cousins?) All but two of these ten studies are purely expository, though Király and Freudenberger offer some welcome speculation.

Arranged chronologically, the studies cover the period 1780 to 1914 and range unevenly in length from a fifty-eight-page mini-monograph to an elaborate thirteen-page footnote. Hans Wagner offers for the first time in any language an analysis of Maria Theresa's pension payments and their suspension by her son. He argues that Joseph II's replacement of his mother's pensions with a legally regulated system of caring for state servants and the poor was an important step toward the creation of the modern welfare state. This chapter is followed by a re-examination by Béla Király of Napoleon's Hungarian Proclamation of 1809 in the light of new materials. The author tentatively concludes that many more Hungarians of all classes were pro-Napoleon than has been generally supposed. Wayne Vucinich's long study of the background and genesis of Croatian Illyrism is simply the best available in English and the outstanding chapter in this book. One of the essay's virtues is that it places the subject not only in the larger context of Habsburg and Hungarian political and social realities, but, more important, in relation to contemporary European developments.

Other chapters also present original interpretations and new material. Herman Freudenberger provides a brief evaluation of several progressive Bohemian and Moravian aristocrats. He speculates convincingly that had they been more numerous, they might have been able to prevent the disintegration of the empire. In addition to presenting much new detail regarding the Romanian national movement during the mid-nineteenth century, Keith Hitchins shows the evolution and development of a workable national program, stressing the conflict between Romanian intellectuals and ecclesiastics. Adam Wandruszka contributes an interesting, extended note to the study of Austro-American relations built around a tragic love affair between an Austrian officer and an American widow. Stanley B. Winters next contributes a useful study of Slavic orientation in Czech political life and thought during the late nineteenth century, focusing mainly on Austro-slavism, pan-Slavism, and Russophilism. Solomon Wank reinvestigates the old problem of change versus lack of change in Austria though a close study of three exchanges between Goluchowski, the "pessimistic" foreign minister, and Aerhanthal, the "optimistic" ambassador to Russia. Then, Erika Weinzierl provides a welcome and attractive examination of the relatively little-known national aspirations of the Italians left in the monarchy after 1859 and 1866, focusing on the question of an Italian university. The final chapter, by Joseph Held, centers on an early twentieth-century Hungarian journal, *Nyugat*, and its supporters who fought for artistic freedom and the autonomy of literature against political interference.

It is a sad commentary on the production of books today that even with photoreproduction this commendable book is so expensive that it will be purchased mainly by institutions. The editors, however, are to be congratulated for having conceived this book and for having guided it so well through publication.

STANLEY B. KIMBALL
*Southern Illinois University, Edwardsville*

ZBYNĚK ZEMAN. *The Masaryks: The Making of Czechoslovakia.* New York: Barnes and Noble. 1976. Pp. 230. $16.50.

This is a double biography of Thomas G. Masaryk (1850–1937), the first president of Czechoslovakia (1918–1935), and of his son Jan Masaryk (1886–1948), who served as Czechoslovak ambassador to Washington (1919–1922) and London (1925–1938), and as foreign minister in President

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Edvard Beneš's exile government in London (1940–1945) and postwar government in Prague (1945–1948). Zbyněk Zeman is a Czech exile in Britain and author of numerous historical studies on the period of World War I.

In the foreword of his book, he complains that the picture of the elder Masaryk drawn in Western literature is "curiously flat." Unfortunately, he adds no new dimension to it. This is an impressionistic portrait of the Masaryks, seriously lacking in depth. Zeman's sketch of President Masaryk is based on a slim selection of published Czech secondary sources. These, though voluminous, by no means exhaust the subject. After the establishment of the Czechoslovak Republic (1918), of which Masaryk was the principal architect, he became an object of an extravagant personality cult which discouraged critical examination of his life and work. Only his early career (up to 1886) has been thoroughly explored by Zdeněk Nejedlý, a Czech art historian who was once an admiring student of Masaryk's but later became a Communist critic. However, precisely because Masaryk was an object of popular adulation, every scrap of information about him was carefully collected and deposited in the Masaryk Archive in Prague. This storehouse of information about the famous Czech philosopher-statesman has survived, but unfortunately political circumstances have prevented historians from fully and freely exploring it. In 1939 Czechoslovakia was occupied by the Germans and in 1948 it was communized. Neither the Germans nor the Communists have had reason to love and honor Masaryk. Except during the early stage of the Communist regime when party hacks sought to "debunk" Masaryk and during the reform period culminating in the famous Prague Spring of 1968 when a few courageous writers sought to rehabilitate him, Masaryk has been relegated to the position of an Orwellian nonperson in his native country.

Jan Masaryk has fared even worse in historical literature. No Czech biography of him has been published, and in the West interest in him centers almost exclusively in the mysterious circumstances of his death after the Communist takeover in 1948—that is, whether it was a result of murder or suicide. An American journalist, Claire Sterling, published an elaborate report on his death (*The Masaryk Case* [1968]), but failed to dispel the mystery.

Zeman's sketch of Jan Masaryk is based almost completely on two admiring posthumous tributes to him written by two of the late foreign minister's close friends, the British diplomat and writer Bruce Lockhart and the American novelist Marcia Davenport. These portraits of Jan Masaryk scarcely deal with his political career, however, and Zeman has made no attempt to supplement them with information scattered in Czech publications. Nor has he explored public and private British archives, which surely contain many a reference to Masaryk, since virtually his entire political career unfolded in London.

Zeman's portrait of the Masaryks is engagingly written and may be profitably read by the nonspecialist. The Masaryks, however, still await their scholarly biographer.

VICTOR S. MAMATEY
*University of Georgia*

VLASTISLAV LACINA. *Krize Československého zemědělství, 1928–1934*. (The Crisis of Czechoslovak Agriculture, 1928–1934). (Práce oddělení novějších československých dějin. Series 1, number 1.) Summary in German. Prague: Ústav československých a světových dějin ČSAV. 1974. Pp. 352.

Vlastislav Lacina's book, one of the few monographs by contemporary Czechoslovak historians dealing with interwar economic history, provides a detailed description of changes in the development and structure of Czechoslovak agriculture during the Great Depression. It contains many tables compiled from official statistics as well as from other sources less readily available in the West. Most of the data presented cover only the years from 1920 to 1933–34, leaving the reader with an incomplete picture as to how far the economic revival progressed before the dismemberment of the Czechoslovak Republic in 1938.

The book begins by discussing the development of Czechoslovak agricultural production and trade during the first, relatively prosperous, interwar decade. Before the onset of the Depression, Czechoslovakia was one of the world's largest exporters of beet sugar, hops, malt, and barley, importing mostly staple products such as wheat, flour, and meat. He argues that the return of agriculture to the pre-World War I pattern of production and trade in the 1920s was at the root of the difficulties during the 1930s, a thesis open to dispute. Lacina divides the general development during the period 1928–34 into three stages. In the first stage, 1928–30, the agricultural crisis began following the collapse of the beet sugar trade in 1927. In the second stage, 1930–33, the rapid fall of grain and other agricultural prices generalized the impact of the crisis over the entire farm sector and sharply reduced incomes. In the third phase, 1933–34, the state initiated larger-scale intervention to support farm incomes.

Subsequent chapters provide a large number of details dealing with the collapse of sugar beet farming, the grain problem, changes in the crop mix, and the shift toward agricultural autarky. The last chapter deals with the impact of the Depression on the profitability of farms, farm investment, and farm indebtedness.

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The main thesis of the book is that the weakness and low profitability of the small farms (up to about fifty acres) made these farms suffer much more severely under the impact of the crisis than did the larger "capitalist" farms. The author argues that the small farmers derived most of their income from animal husbandry in which government showed little interest. Governmental support went, instead, to maintaining the price of grain—the market crop of "capitalist" farmers. He concludes that the failure of liberal capitalism to solve these deep-seated problems paved the way for later socialist collectivization of agriculture.

Although scholars should welcome a new book on Czechoslovak agricultural history, this study has many weaknesses. Numerous selective details and repetitive discussions make it difficult to follow Lacina's arguments and ideas. Data are often fragmentary and likely to support statements rather than lend themselves to a meaningful analysis, and he makes no attempt to present the problem in the wider context of the national economy as a whole. The regrettable absence of any aggregate data, such as total agricultural output and farm incomes, and an index reduces the usefulness of the book to the specialized reader.

ZORA PRYOR
*Temple University*

F. GREGORY CAMPBELL. *Confrontation in Central Europe: Weimar Germany and Czechoslovakia.* Chicago: University of Chicago Press. 1975. Pp. xvi, 383.

Following a dozen years of apparently friendly relations between Germany and Czechoslovakia through the twenties, the two states became the leading antagonists in Central Europe in the thirties. Until now, the 1918–35 period of their diplomatic relations has not been thoroughly examined; F. Gregory Campbell's competent and concisely written book now fills this gap. It will be of interest to specialists in both European diplomatic history and Central European history.

The author took advantage of the recently opened German, British, and French documentary sources to produce a detailed study of the background and development of German-Czechoslovak relations, broadened by taking into account economic factors and public opinion. The archives of the Czechoslovak Ministry of Foreign Affairs and the Masaryk and Beneš Archives are still closed to Western scholars; only fragments of these collections have been published. Campbell supplements them by using German, Austrian, and Czechoslovak newspapers.

The book's main thesis is that the focal point of Czechoslovak foreign policy after World War I was the ever-present danger of a *grossdeutsch* organization of Central Europe. In establishing their state the Czechs had to cope with the problem of the German minority in the historic crownlands of Bohemia and Moravia, and the foreign policy of Czechoslovakia sought security against a powerful Germany. Germany's policy evolved into one of carefully avoiding overt intervention in Czechoslovak affairs but gave practical support to the Germans of Bohemia. Czech leaders hoped that the temporarily helpless—but potentially dangerous—neighbor could be made to understand that acceptance of and peaceful relations with the new state were in Germany's interest, and followed a policy of economic conciliation.

Masaryk and Beneš, anticipating the time when Germany would resume a dominant position in Europe, adhered to a policy of restraining Germany through the Entente and the collective security system of the League of Nations. But attempts at monarchical restorations in Austria, Hungary, and Germany, which were considered serious threats to the existence of Czechoslovakia, drove Beneš into close political and military alliance with France, thus linking the future of Czechoslovakia exclusively to one ally. The prospect of the disintegration of the Weimar Republic during the reparations crisis and the rise of Nazism in Bavaria supported the Germans of Bohemia in refusing participation in the government of Czechoslovakia and raised hopes for an *Anschluss*.

The fear of Germany remained the *Leitmotif* of Czechoslovak foreign policy: Beneš worked at the drafting of the Geneva Protocol and after its failure foresaw dark days for Europe. Campbell perceives the treaty of Locarno as a foreshadow of the Munich Agreement: France secured her Western frontier but dropped requests for guarantees of the boundaries of her Eastern ally. Under the cover of "friendly" relations Stresemann sought to discredit the Czechs wherever possible and to create a rift between France and her Eastern ally. In order to divert Germany's attention, Masaryk and Beneš tried to turn German revisionism toward Poland, thus deepening hostility between the two Slavic potential allies in East-Central Europe.

Germany's aim was economic penetration of Central Europe: domination without an *Anschluss* and without forfeiting the "Locarno spirit" and Germany's new respectability. Beneš opposed a German economic bloc in Central Europe and sought independent economic development for Czechoslovakia by preferential tariff treaties among the states of the Little Entente, Austria, Hungary, and possibly Bulgaria and Greece. German diplomacy worked to disrupt wherever possible his plan of economic and political consolidation of the non-German Central and East European states, but it kept away from an aggressively revisionist policy in Central Europe.

Weimar basically accepted the legality of the

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Versailles Treaties; by the late 1920s the issue of the Sudetendeutsche was regarded as settled, and German Bohemians participated in the Czechoslovak government. The author concludes that "had the relatively prosperous and peaceful atmosphere of the late 1920s endured, it is likely that understanding and toleration between the Czechs and the Sudeten Germans would have developed further" (p. 209). This process of consolidation was stopped abruptly by the world economic crisis and the rise of political radicalism in Germany.

Britain was willing to make commitments only as far as the Rhine and was suspicious of and hostile toward Czechoslovakia. The British Ambassador in Prague considered Czechoslovakia to be "a fictitious country founded on several injustices." He hoped that Europe would once more be handed over "to those who were competent to run it," and the people of the successor states be relegated to "scrubbing floors and cleaning windows" (p. 247). By 1932 even France neglected her ally and the collective security system was dead. The conclusion of the Four Power Act signaled the demise of the Versailles system, the foundation on which the Czechoslovak state was built. Newly radicalized Sudeten Germans flocked to Henlein's German-financed *Heimatfront*, which in 1935 emerged as the strongest parliamentary unit in Czechoslovakia. The period of subversion of the Czechoslovak Republic was under way.

Campbell does not see the period of the two liberal republics, Weimar and Czechoslovakia, as one of reconciliation, different from the nationality conflicts of the late Hapsburg empire and the brutality of the Third Reich. On the surface it seemed that common interests of the republican progressive regimes would overcome national differences in Central Europe, but the basic conflict persisted between the Germans' desire for resurgence and the Czechoslovaks' efforts to establish the independence and sovereignty of their country.

DAGMAR HORNA-PERMAN
*Georgetown University*

BRIAN LARGE. *Martinů*. New York: Holmes and Meier Publishers. 1976. Pp. xiv, 198.

This all too conventional biography of the Czech composer Bohuslav Martinů (1890–1959) makes a fuller contribution to musicology than to cultural history. Martinů's life is chronicled, his compositions catalogued, and his opinions noted, but his career is not placed in any extramusical context. Brian Large interprets Martinů the composer as a quasi-impressionist, who was influenced most deeply by Albert Roussel in Paris. To adapt a term from Paul-Gilbert Langevin's *Le Siècle de Bruckner* (1975), Martinů is what might be called an "ethnic impressionist." Large reiterates that although Martinů spent seventeen years in Paris and slightly longer in America, he remained "totally Czech to the very end" (p. 156). Unfortunately, nowhere does Large buttress this thesis by evoking "Czech" qualities or by discussing the institutions and mores which prevailed in the Habsburg Bohemia of Martinů's youth. The author might learn from Langevin's book how cultural history and musicology can interact to illuminate a wide range of issues. Whereas Large focuses exclusively on the music, Langevin forges categories such as "ethnic romanticism" that increase understanding not only of musicians but of artists and writers as well.

The most haunting memory from this book is the description of how Martinů spent most of his first six years in his parents' rooms atop the church tower of their village. When the six-year-old had to leave this perch to attend school, he felt like a celestial being descended among Lilliputians. What a compelling image this childhood in a belfry provides—an image both for the alienation of the artist from society and for the growing separation of one academic specialty from another. Martinů's childhood spent looking down from a tower seems to foreshadow the entire course of twentieth-century creativity, as well as the frustrations of his career and the failings of this book.

WILLIAM M. JOHNSTON
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V. MAAMÄGI *et al.*, editors. *Eesti NSV ajalugu*. Volume 3, *1917. aasta märtsist kuni 50-ndate aastate alguseni* [History of the Estonian SSR; From March 1917 to the Beginning of the 1950s]. (Eesti NSV Teaduste Akadeemia, Ajaloo Instituut.) Tallinn: Kirjastus "Eesti Raamat." 1971. Pp. 754. 4r. 79k.

Beginning in the 1950s, the regional academies of sciences in the Soviet Union launched multivolume general histories of the non-Russian republics. This volume, covering the period March 1917–1950, is the third and final one in the series on Estonia. All three volumes have appeared in both Estonian (1955, 1963, and 1971) and Russian (1961, 1966, and 1974), with generally minor changes in the Russian editions. The entire series represents a collective effort, and volume 3 includes forty authors from the Estonian Academy of Sciences, the Party Institute of History, and various institutions of higher learning.

The book contains three major sections—the Revolution and Civil War (1917–1920), the interwar republic (the "restoration of capitalism"), and Soviet Estonia (1940–1950)—with over sixty percent of the space devoted to the first and third parts. This emphasis reflects the predictable major theme, namely the inevitability of the triumph of Soviet power in Estonia under the leadership of the

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Communist Party. The "bouregeois" republic of the 1920s and 1930s was only a temporary setback; nothing could stop the inexorable march of history.

The most exasperating aspect of this book is its self-righteousness coupled with a totalistic point of view. All the opponents of the Bolsheviks in the early years, including the Estonian bourgeoisie, the Mensheviks and SR's, and the Baltic German nobility, unhistorically appear together as coconspirators against progress. For the 1930s, the authors depict virtually all noncommunist political forces as fascist or cryptofascist. Indeed, they suggest that the "bourgeois" republic was the incarnation of evil, while the establishment of Soviet power initiated a secular paradise. Throughout the book they systematically avoid facts that would blemish the Bolshevik record. Although an extensive description of terror in the Civil War appears, none of it is associated with the Bolshevik side. Totally ignoring the role of Zhdanov and Soviet military force, the authors picture the upheaval of 1940 as a spontaneous domestic revolution. They gloss over or do not mention the Soviet deportations of the 1940s.

The book is filled with figures but, with one lone exception, no sources are ever cited to support them. In fact, the only other footnotes refer to the works of Lenin (thirty-nine times) and once each to Marx and Engels and the Twelfth Party Congress. The authors provide an extensive bibliography, but they list only Soviet secondary sources. Historiographical surveys in the text dismiss non-Marxist works as falsifications, but one welcome feature is an exhaustive index of names.

TOIVO RAUN
*California State University, Long Beach*

MICHEL LARAN and JEAN SAUSSAY, editors. *La Russie ancienne, IX^e^–XVII^e^ siècles*. With a preface by FERNAND BRAUDEL. (Collection Documents pour l'Histoire des Civilisations.) Paris: Masson et Cie. 1975. Pp. 335. 70 fr.

In the foreword to this collection of documents by Michel Laran and Jean Saussay, Fernand Braudel writes that this book is remarkable. Remarkable not just for its excellent critical notes, bibliography, numerous maps, or illustrations, but because the readings enable the reader, aided by informed introductory remarks, to immerse himself in the centuries of Russian history and to conjure up a vivid portrait of medieval Russian civilization.

Unlike many readers available in English, this book is thematically organized. In the tradition of the journal *Annales: Économies, Sociétés, Civilisations*, Laran and Saussay eschew narrowly conceived political, diplomatic, and biographical history (*histoire événementielle*), and instead present an outline of the structures and *mentalités* of Russian civilization. The text is divided into four parts covering Kievan Russia, the Mongol Yoke and the Formation of the Muscovite State, Strengths and Weaknesses of the Centralized State (1505–1598), and Muscovy at the Crossroads: Traditions and Changes (1598–1682).

Each of the parts is subdivided into several chapters that illustrate the book's themes. Kievan Russia is viewed against its economic, social, religious, and cultural traditions rather than through the history of its warring princes. The Mongol era is the history of the Mongol conquest, the rise of Moscow, the beginning of serfdom, and the fifteenth-century cultural revival. The crisis in the last third of the sixteenth century is presented as the result of an economic *conjoncture* and Russia's sociopolitical traditions rather than the presumed pathology of Ivan IV. This section includes readings on administrative reforms, the *oprichnina*, economy, religion, and the peasantry. The seventeenth century is seen as a crisis between tradition and modernization with readings on the "persistent troubles" from the 1601–1603 famine to the Stenka Razin rebellion, serfdom, economy, Church Schism, and Western cultural influences.

No collection of documents will satisfy everyone. There are selections from the Stoglav, Domostroi, and the Bolotnikov and Razin uprisings, but the *Zadonshchina* and Lives of Boris and Gleb, as well as Alexander Nevsky are missing. The book's strength, however, lies in its organization, and future editors of Russian historical sources would do well to consult it as a model.

LAWRENCE LANGER
*University of Connecticut*

TERENCE ARMSTRONG, editor. *Yermak's Campaign in Siberia: A Selection of Documents*. Translated from the Russian by TATIANA MINORSKY and DAVID WILEMAN. (Second series, number 146.) London: The Hakluyt Society. 1975. Pp. x, 315. $18.00.

Ermak, a Cossack *ataman*, is closely associated with the beginnings of Russian expansion into Siberia in the 1580s. His significant campaign is presented here in four different historical, although not contemporary, Russian versions. The Strogonov, Esipov and Remezov chronicles are translated in their entirety along with a short relevant passage from the New chronicle. In the three major narratives the editor has selected one basic version for translation and has avoided linguistic problems per se and the numerous variations in manuscripts. Supplementing these four Siberian chronicles are translations of seven related documents for the reign of Tsar Ivan IV.

In the introduction, the editor presents back-

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ground information on Siberia, the peoples who inhabited it, and the Strogonovs. Most valuable, however, is the lucid discussion of problems relating to authorship, versions, dating, and the discrepancies of the chronicles. The editor is to be commended for the breadth of his coverage, especially for the discussion of Ermak as folk-hero, written by William Harrison of the University of Durham.

It is in the illustrations, however, that the book excels. The Mirovich manuscript (the basic version presented in translation here) of the Remezov chronicle was illustrated, and fortunately the 154 illustrations accompanying the text have been reproduced. In addition, there are two detailed maps, a virtual necessity for following the numerous geographical references in the texts.

The editor has unfortunately presented the chronicles in order based upon length, and not in the chronological order one might expect. The reader is cautioned to read the texts in the order presented since issues discussed in the footnotes are not repeated. While there is some awkward phraseology, the translations on the whole read smoothly, and readily accessible, good translations of Muscovite sources such as the volume under review are always welcome.

EMILY V. LEONARD
*Western Illinois University*

NIKOLAI N. BOLKHOVITINOV. *The Beginnings of Russian-American Relations, 1775–1815.* With an introduction by L. H. BUTTERFIELD. Translated by ELENA LEVIN. Cambridge, Mass.: Harvard University Press. 1975. Pp. xviii, 484. $35.00.

WALTHER KIRCHNER. *Studies in Russian-American Commerce, 1820–1860.* (Studies in East European History, number 19.) Leiden: E. J. Brill. 1975. Pp. xii, 265. f 68.

These two studies complement each other chronologically, but exemplify different genres of historical writing. The translation of Nikolai N. Bolkhovitinov's survey presents to the English-reading public a broadly conceived, thoroughly documented account that provides the only systematic treatment of the subject and represents one of the best products of recent Soviet historiography. As the Russian edition has been perceptively evaluated in this journal by Roderick E. McGrew (vol. 73, 1967–68), I shall forego substantive comment and appraise only the translation. The new edition seeks to convey the Russian original, supplemented by some additions and corrections the author made on the basis of a research trip to the United States in 1968 and publications since 1966. A brief, graceful essay by Lyman H. Butterfield introduces the work; the author's own historiographical and bibliographical opening has been transposed to an appendix.

As one who has translated a Russian academic work, I sympathize with the translator's difficulties. Bolkhovitinov's book certainly merits translation; indeed, its quality is all the more impressive considering the USSR's limited research resources for American history, the ideological and political constraints of the subject, and the weak historiographical tradition in the field. It is especially regrettable, therefore, that the translator, editors, and publisher have served their Soviet colleague so poorly. Despite a consistently idiosyncratic usage of the definite article, the translation reads smoothly most of the time; but spotchecks against the Russian reveal a disturbing number of misconstrued passages, omissions, and several glaring errors. Readers will be surprised, for example, to meet "Bishop James Madison" (p. 62), whereas the Russian text identifies him as "the future president of the USA" without any ecclesiastical title. Notwithstanding the author's guidance, the translator and the compiler of the index have created two persons out of one, Princess Catherine Dashkov and Princess E. R. Dashkova, and have transformed the office of the Ottoman minister of foreign affairs into a nonexistent person, Reis Effendi. As an instance of how an omission can confound meaning, consider this quotation from the *St. Petersburg Gazette:* "The refuge in America, proves their weakness and foretells a total freeing of America from the English" (p. 42). In Russian, by contrast, the quotation begins: "the seizure from the English of Rhode Island, which served them as a refuge, proves. . . ." A more ominous case occurs on page 145 where excisions soften a slashing indictment of Western historiography for suggesting similarities between the Decembrists and the early reforms of Alexander I. Numerous other omissions, inexplicable additions, mistranslations, and misprints speckle the English text. Specialists on the subject and era will doubtless find other mistakes. In short, Bolkhovitinov and his colleagues at home and abroad have every right to be shocked at this mediocre translation of his painstaking research.

The title of Walther Kirchner's book accurately conveys its diffuse content, which is supported by 47 tables, 10 appendices, and 10 plates. It is unfortunate, incidentally, that Bolkhovitinov's new book (Moscow, 1975), the sequel to that discussed above, appeared at the same time as Kirchner's, for each work would have benefited from the other. In contrast to Bolkhovitinov's broad coverage of the tumultuous era to 1815, Kirchner focuses upon commercial relations during a rather uneventful "normal" epoch. He discusses trade from intercontinental and international perspectives, investigating diverse aspects of Russian-American

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economic interaction and considering the impact of various noneconomic factors. He purposely excludes, however, trade between Russian America, Siberia, and the western portions of the United States. The author's research into local and private Anglo-American archives is impressive, as is his usage of American and Russian business periodicals. Quanto-historians will welcome the plethora of statistical data, which far surpasses any previous work in this field. Thus Kirchner's chapter on "Statistics: Truth and Fiction" expertly explicates the shortcomings of the data collected during the period, particularly noting the failure to account for bullion transfers, smuggling, governmental purchases, and earnings from commissions. He also analyzes entrepreneurship and studies several American firms in Russia and at home, as well as a few Russian ones.

Yet the book as a whole seems less successful than some of its parts. Although the author poses some provocative questions and suggests several generalizations, the subject remains rather dry. The presentation short-changes the Russian side of the relationship, underestimates the significance of technological change in some instances—e.g., the advent of steamships and the development of cotton spinning—and repeats some dubious explanations of Russian economic behavior. No Russian manuscript material has been used. Sporadic stylistic infelicities mar the presentation, as do numerous typographical errors—perhaps because the book was printed in Hungary. This is a work for the devoted specialist in economic history.

J. T. ALEXANDER
*University of Kansas*

A. M. SOLOV'EVA. *Zheleznodorozhnyi transport Rossii vo vtoroi polovine XIX v.* [Railroad Transportation in Russia in the Second Half of the 19th Century]. (Akademiia Nauk SSSR, Institut Istorii.) Moscow: Izdatel'stvo "Nauka," 1975. Pp. 314. 1 r. 60 k.

The purpose of this work is to tell how railway-building in Russia in the second half of the nineteenth century was an essential part of that country's industrial revolution, which, in turn, was the precondition for the Revolution of 1917. By relating the development of railways to Russia's overall economic development, as well as giving an account of government policy and financial matters, A. M. Solov'eva has produced a work of value.

For Russian railroad transport before 1861 the author draws very heavily on pre-1953 Soviet scholars and offers little in the way of new facts, interpretations, or sources. The book concentrates, however, on the period 1861–1900. This section is divided chronologically into three parts. The author relates that in the 1860s and 70s the Russian government entrusted the bulk of construction to private entrepreneurs, both foreign and Russian, to whom financial aid in various forms was given and that this "concession system" was haphazardly planned and plagued by corruption and inefficiency. She shows, nevertheless, that by 1880 Russia had a railway network linking major population centers and the main agricultural areas with Baltic and Black Sea ports, resulting in an increase of the grain export trade.

In the second period, the 1880s and early 1890s, the rate of construction slowed, but Solov'eva notes that the government began to exercise increasing supervision over private railway companies and initiated a policy of buying many lines and building some new ones at state expense, including a line to link the coal and iron deposits of southern Russia.

In the third period, 1893–1900, there was another "boom," with railways being regarded as a vital factor in the rapid industrialization of Russia sponsored by Finance Minister S. Witte. Solov'eva shows that this construction contributed greatly to the growth of Russian heavy industry by stimulating the demand for its products and by tapping new sources of raw materials.

Relying mainly on pre-1917 Russian printed sources and some archival materials (but neglecting contemporary newspapers and statutes and all non-Russian sources), Solov'eva has written a work that is more comprehensive, balanced, and objective than any previous treatment of tsarist Russian railways by a Soviet scholar. And in contrast to the histories of railways by Verkhovskii and by Kislinskii published before 1917, she emphasizes the results of railway construction on the economic development of Russia. Although the details of construction and operation are relatively neglected, excellent maps and tables give the reader a clear concept of the growth of the railway network. In short, this book gives a concise summary of the development of Russian railways before 1900. However, a detailed and definitive history, based on a full range of primary source materials, is yet to be written.

RICHARD MOWBRAY HAYWOOD
*Purdue University*

HAROLD N. INGLE. *Nesselrode and the Russian Rapprochement with Britain, 1836–1844.* Berkeley and Los Angeles: University of California Press. 1976. Pp. xi, 196. $11.75.

Generations of scholars of virtually every historical school and political persuasion have either dismissed Nesselrode as the creature of Nicholas I or, more commonly, condemned him for his alleged neglect of Russian national interests and for the ultimate failure of his policies. Viewed by many of his contemporaries as too "cosmopolitan," as na-

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ively Anglophile, and at various times as either pro-Austrian or pro-German, Nesselrode advanced a broadly European concept of the balance of power and the community of great power interests in an age when resurgent nationalism made such theories highly unpopular. His reputation was tarnished further when his critics, who viewed Britain as Russia's major rival, were proven "correct," and Nesselrode's policy of rapprochement failed to avert the disastrous Crimean War.

Harold Ingle's purpose is not so much to defend Nesselrode as to present a balanced examination of the diplomatic events and policies as they occurred and to interpret those events and policies in the context of Nesselrode's own ideals. The first third of the book is simply a chronicle of Nesselrode's early career and of diplomatic developments in the reigns of Alexander I and Nicholas I. The body of the work presents a detailed account of Anglo-Russian relations in the critical period from 1836 to 1844, when Nesselrode sought not only to prevent crises from erupting but to preserve an atmosphere in which close ties between the two great powers could develop. Unlike his critics, Nesselrode was deeply convinced of the identity of Russian and British interests in preserving peace. Nesselrode's European orientation clashed strongly, however, with the xenophobic outlook and anti-European sentiments of the supporters of the dominant theory of "Official Nationality."

Although this volume breaks no new ground either in its intepretation or its use of sources, it is a well-crafted case study of a significant phase of Russian diplomatic history.

JUDITH C. ZACEK
*Albany, New York*

N. IA. EIDEL'MAN. *Gertsen protiv samoderzhaviia: Sekretnaia politicheskaia istoriia Rossii XVIII-XIX vekov i Vol'naia pechat'* [Herzen against Autocracy: The Secret Political History of Russia in the 18th and 19th Centuries and the Free Press]. Moscow: Izdatel'stvo "Mysl'." 1973. Pp. 366.

The theme of this book is what the author calls the secret history of Russia in the eighteenth and the first half of the nineteenth centuries. The phrase "secret history" designates a large number of frequently interconnected themes, episodes, and events, which the mid-nineteenth-century Russian government wished to conceal from outsiders (and from its own subjects) or distort beyond recognition. Part of the struggle of the great Russian émigré radical, Alexander Herzen (and various colleagues), was to reclaim what they regarded as the "real" past, a process which Herzen believed would gradually reveal a new and different Russian history, unmask the autocracy, and thus bring closer the day when revolutionary change would come to Russia. Thus, *Herzen Against the Autocracy* is about what radical American historians not so long ago called "the search for a usable past": the discovery, creation, and elaboration of that past which will support one's politics in the present and bear up one's hopes for the future.

Herzen's ammunition in his struggle with the government (which was actively propagating *its* version of the past) were the numerous historical documents, memoirs, essays, treatises, and other material which he published during his London exile. The greater part of the material appeared between 1855 and 1865. The *Memoirs* of Catherine the Great, Radishchev's *Journey from St. Petersburg to Moscow*, Prince Shcherbatov's *On the Corruption of Morals in Russia* are among the more famous items unearthed and put into circulation by Herzen's Free Russian Press. Eidel'man divides this material into two categories, although they are by no means watertight. First, there is material exposing the moral and political corruption of the Russian elite: documents describing Peter the Great's murder of his son; letters and other material questioning the legitimacy of the Emperor Paul; descriptions of the government harassment of Alexander Pushkin and possible involvement in his death. The second category is comprised of material describing the resistance of Russian society to the policies of the autocracy: Pushkin's historical studies of the Pugachev rebellion; the memoirs of the Decembrists.

Eidel'man is an impressive scholar: erudite, witty, and professional to the core. There is scarcely a trace of cant or pedantry in his work. He is the author of numerous articles and several book-length studies: *The Secret Correspondents of the "Polar Star"* (1966) and *Lunin* (1970).

*Herzen Against the Autocracy* is a delight to read. Although the formal unity of the book is provided by Herzen's contest with the autocracy for Russian history, Eidel'man dwells lovingly on the development of each of the case histories into which the book is divided, and in the process he touches upon a number of fascinating minor issues of Russian history: the possibility (highly unlikely) that Nicholas I committed suicide; the question of who wrote the celebrated lampoon on Pushkin (inducting him into the Most Serene Order of Cuckolds), a slander which played a considerable role in his duel and death. Only the language barrier will prevent this book from attracting a considerable audience of readers with a general interest in nineteenth-century Russian and European history.

ABBOTT GLEASON
*Brown University*

DANIEL R. BROWER. *Training the Nihilists: Education and Radicalism in Tsarist Russia*. Ithaca, N.Y.: Cornell University Press. 1975. Pp. 248. $12.50.

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Daniel R. Brower's interest in education and radicalism began with a study of the Russian intelligentsia. By the end of that study he came to the conclusion that the term "intelligentsia" had lost its analytical punch and had best be discarded. The dismissal of this grand old word launched our author on a career of destruction. The power of his book is its destructive force. Before he is through, he shows the thinness of every favorite explanation for the rise of student radicalism in tsarist schools a century ago.

His instrument of destruction is an intensive study of police records on 405 radicals active in the St. Petersburg area between 1840 and 1870. His meticulous exercise in social profiling revealed no appreciable difference between the radicals as a group and the student population as a whole. At this point the book shifts from carefully focused demolition to loose reconstruction.

The author's explanation for the high incidence of students in the early revolutionary movement rests not on the hard scrabble of quantifying archival records, but on the very memoir literature he has previously disdained. Since there is nothing in the backgrounds of the radicals to distinguish them from the students as a whole, the reason for the recruitment of students into the revolutionary movement must lie in the educational experience itself. Brower discovers a "school of dissent" where the nihilists are trained. This wild school mushrooms, as near as I can make out, in the interstices between lecture hall, rented rooms, and basement printing presses. It grows like a fungus on the student community, where the impatient sons of provincial gentry-officials, away from family, packed in close communal quarters in churning metropoli, ingest the habits of group as opposed to mere individual disgust with a world that makes the young wait in line for its rewards.

But Brower's gifts run to demolition. He even demolishes the correct name of Nicholas I's minister of education. And so, in due course, he even destroys his own drawn-out argument for an operative "school of dissent" as thoroughly as he had previously demolished the familiar edifices of poverty, alienation, generation gap, upward mobility, and ideological infection. Since all students, by his own count, had equal access to the "school of dissent," what we really want to known and never find out is why some entered its portals and most did not. Like any bit of explosive material, this ingenious, fascinating, and occasionally fatuous volume should be used with care.

As for the original question of the relationship between students and radicalism, I modestly suggest that no other statistically visible population pool possessed such a concentration of the leisure, the mobility, the verbal and motor skill needed for the early nihilistic style of revolutionary sport as played with the equally amateurish tsarist police.

PATRICK L. ALSTON
*Bowling Green State University*

ANDRZEJ WALICKI. *The Slavophile Controversy: History of a Conservative Utopia in Nineteenth-Century Russian Thought*. Translated by HILDA ANDREWS-RUSIECKA. New York: Oxford University Press. 1975. Pp. viii, 609. $39.00.

The English translation of Andrzej Walicki's *W kręgu konserwatywnej utopii* is a major event for all those interested in Russian intellectual history. The greater accessibility which the English edition will ensure should confirm Walicki's place as a scholar of the very first importance and highest achievement.

Originally published in 1964, this monumental work is much more than a study of classical Slavophilism. Walicki ranges far back into the eighteenth century, examining in particular the writings of Shcherbatov and Karamzin in order to trace the origin and development of the ideas which were welded into what he calls a *Weltanschauung* (as opposed to an "'ideology,' more narrowly conceived) between the late 1830s and the 1850s. The author goes on to trace, with thoroughness and lucidity, the impact of the Slavophile synthesis on Russian intellectuals in the second half of the nineteenth century. He demonstrates convincingly how central Slavophile ideas were, not only for the later "philosophical Right" (Grigoriev, Dostoevsky, the Panslavs, Leontiev), but for Herzen and the subsequent development of Russian Populism. He leaves the reader at the threshhold of the twentieth century, but points out the continuing power of Slavophile formulations over a new generation, including Berdiaev, Bulgakov, Frank, etc. Walicki's range and grasp are enormous, but perhaps his most impressive achievements are to make us confront again the seamless totality of pre-Revolutionary Russian culture and to restore to the conservative utopians (as he calls them) the centrality which they deserve.

Walicki's methodology is derived from the work of Karl Mannheim; *The Slavophile Controversy* is the sociology of knowledge at its best. There are those, of course, for whom Walicki's social interpretation of ideas will not, finally, be convincing. The difficulties of his method are particularly striking, for instance, when he writes that Tolstoy's Christian anarchism was a "direct reflection of the social *Weltanschauung* and (partially) of the interests of the exploited and ruined masses of the patriarchal peasantry." It is not only Tolstoy scholars who will raise their eyebrows at this formulation! Still, no fair-minded scholar can deny the breadth, depth, and seriousness of Walicki's achievement.

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It is a major synthesis which will be a source of insight and controversy among scholars for a long time to come.

Despite a few errors and awkward places, Hilda Andrews-Rusiecka's translation is accurate and readable, but there are a lamentable number of typographical errors.

ABBOTT GLEASON
*Brown University*

JEREMIAH SCHNEIDERMAN. *Sergei Zubatov and Revolutionary Marxism: The Struggle for the Working Class in Tsarist Russia.* Ithaca, N.Y.: Cornell University Press. 1976. Pp. 401. $18.50.

There are few aspects of tsarist labor history more intriguing or controversial than the Zubatovshchina, the government's policy toward the working class from 1901 to 1903. Sergei Zubatov, a shrewd chief of the security police, formulated the policy for the purpose of deflecting workers from the revolutionary movement by enlisting them into officially sanctioned unions committed solely to the proletariat's economic and cultural advancement. The Russian workers, reasoned Zubatov, were instinctively conservative and loyal to the tsar and would eschew politics so long as the government took measures to alleviate their wretched plight.

Opponents of the autocracy considered the entire police enterprise cynical as well as threatening to their endeavors to change the country's political system. Therefore they were unsparing in their condemnation of everyone who collaborated with Zubatov. The polemics had not subsided even in the early 1960s, when emigré socialists in the United States still engaged in passionate debates over the success of Zubatov's policy and the extent and nature of his support among the proletariat.

Jeremiah Schneiderman has written the first satisfactory study in depth of the Zubatovshchina. The principal virtues of his work are comprehensiveness and scholarly detachment. After describing Zubatov's conversion from a young revolutionary to a dedicated police official, the author painstakingly traces the vicissitudes of the daring policy in several major cities. Sizable numbers of workers joined the unions sanctioned by the police, and in Minsk and Odessa many Jewish workers were for a brief period strongly attracted to the nonpolitical organizations. In part, Zubatov's success may be attributed to his genuine concern for the well-being of the proletariat: on occasion he went so far as to urge strikers to persist in their demands and even contributed money to their strike funds. Indeed, there is merit to Schneiderman's contention that the police program "was well tailored to fit the needs and interests of a Russian working class then in an early stage of development, indifferent to politics, loyal to the throne, and desirous, above all, of receiving tangible improvements in living and working conditions."

Still, as the author acknowledges, in autocratic Russia the Zubatovshchina was bound to fail. Although some high officials supported the endeavor, most of the leading bureaucrats dreaded any form of working-class organization, which, they believed, would inevitably become an uncontrollable, destructive force. Sergei Witte, the powerful minister of finance, led the opposition to Zubatov, and he was ardently supported by industrialists. Moreover, the government's anti-Semitism was so pervasive that Jewish workers could not long remain active in unions promoted by the authorities. In consequence, the Zubatovshchina never evolved into a consistent and comprehensive program and did not fundamentally affect the development of the Russian labor movement, though it should be noted that the Gapon movement, so critical in the Revolution of 1905, started out as a police union.

The contents of this study are intrinsically interesting, but unfortunately Schneiderman's style does not convey much drama or excitement. His writing is often stilted, he is repetitious, and he tends to dwell excessively on detail. Nevertheless, this is an informative and solid book that deserves to be widely read.

ABRAHAM ASCHER
*Brooklyn College, and National Endowment for the Humanities*

ANN ERICKSON HEALY. *The Russian Autocracy in Crisis, 1905–1907.* Hamden, Conn.: Archon Books. 1976. Pp. 328. $15.00.

Ann Healy's book concentrates on the brief but crucial period between the granting of a semi-constitutional system on October 17, 1905 and the dissolution of Russia's first legislative assembly (the Duma) in July 1906. At present, there is no scholarly work in English on the First Duma. Because she was unable to use the abundant relevant Soviet archival material, Healy has not filled the gap. There remains a need for a good summary for the nonspecialist, but this work is marred by a marked antirevolutionary bias which diminishes its use for the reader not already familiar with these events. "Leftist propaganda screeched" (p. 28). The head of the Petersburg Soviet was "unkempt" (p. 31). Trotsky was a "master of fiery political oratory" (p. 31), and, in the hideously exploitative relations between employers and workers, "there were grievances on both sides" (p. 28).

Healy's non-specialist readers will get the in-

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correct impression that all Russia revolved around the liberal Constitutional Democratic Party (the Kadets) after 1905. Perhaps her exclusive reliance on the Kadet daily newspaper *Rech'* explains this false image. Besides the forgivable absence of archival sources, the author has also ignored or underutilized several important published Russian-language works. Despite these problems, the book contains useful accounts of the government's elaboration of the new system and the activity of the First Duma itself. The sections on the background of the revolution and on the election process are, however, shallow and clichéd.

One still must ask why the government failed to cooperate with the new Duma? Healy leaves us only with repeated statements about difficulties of communication in a vast land and the inability of uneducated and inexperienced masses to make "rational" choices. The peasants of Russia elected to the first two Dumas the most radical candidates available in order that they might be given the land they so needed. Is that really so irrational?

ROBERT EDELMAN
*University of California, San Diego*

A. L. SIDOROV. *Ekonomicheskoe polozhenie Rossii v gody pervoi mirovoi voiny* [The Economic Situation of Russia in the First World War]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1973. Pp. 654.

Russia's shortsighted War Ministry confined its prewar planning efforts largely to matters of troop deployment. It ignored the equally critical task of ensuring adequate industrial support. Hence, already by September 1914, industry could supply the army with only 20 to 30 percent of the combat provisions it required, but the ministry "continued to work at a prewar tempo, directed by no one." A. L. Sidorov's volume has two purposes: first, and most important, to provide an extremely detailed analysis of the main result of this negligence, the pervasive economic crisis that beset the country throughout the war; and second, to denounce those influential industrialists of the Russian "bourgeoisie" who continued to promote their own economic self-interest without regard for the social or military consequences.

Sidorov argues that "bourgeois" pursuit of personal gain and aversion to state planning or regulation rendered emergency wartime organizations such as the Special Councils ineffective. Inept, "incapable of . . . bold initiatives or creativity," the industrialists opposed creation of large-scale, state-run supply enterprises, kept production and distribution of critical commodities such as oil beyond the scope of state control, and even refused to take measures against bank speculators. Consequently, the Special Council on Defense made only "perfunctory and haphazard" recommendations, while the War Industry Committees, described as "a big political game," created an exaggerated image of their importance but accomplished little of practical value.

Study of the wartime economy was a lifelong endeavor for Sidorov. His first article on the subject appeared in 1927. This volume, published seven years after his death, is the most detailed and comprehensive account of Russia's productive deficiencies. Based on a wide array of archival sources, his arguments are often convincing. In addition, he provides valuable new information about the Special Councils and their failure to act decisively. Nevertheless, Sidorov's portrayal of the bourgeois leaders is disturbingly predictable; it coincides with the views of other Soviet scholars working on the prerevolutionary period and the book at times takes on the tone of an indictment. A more compact treatment of the period, with similar conclusions, is V. S. Diakin's *Russkaia burzhuaziia i tsarizm v gody pervoi mirovoi voiny* (Leningrad, 1967), which is more concerned with the "bourgeois" political parties, the Union of Towns, and the Union of Zemstvos.

MICHAEL F. HAMM
*Centre College*

## NEAR EAST

SALAHI RAMSDAN SONYEL. *Turkish Diplomacy, 1918–1923: Mustafa Kemal and the Turkish National Movement.* (Sage Studies in 20th Century History, volume 3.) Beverly Hills, Calif.: Sage Publications. 1975. Pp. xv, 267. $13.50.

In recent years two rich sources on the period of Turkey's national renaissance have become available—the British Foreign Office archives and the published memoirs of prominent Turks. Salahi Sonyel has used these materials to good advantage in articles in Turkish periodicals and in a volume on 1918–1920, *Türk Kurtuluş Savaşi ve Diş Politika* (Ankara, 1973). He now offers an English account of Mustafa Kemal's diplomacy during the nationalist struggle in 1919–1923.

First the author traces the evolution of the nationalist movement, its foreign contacts, and its relations with the sultan's government in Istanbul down to the establishment of the Ankara government in April 1920. Sonyel then deals separately with the "eastern" and "western" policies of the Kemalists, as they negotiated with Russia and with the western Entente powers, until the Russo-

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Turkish treaty of March 1921. Finally, he follows relations with all powers to the defeat of the Greek invasion, and then through the Mudanya armistice and the Lausanne peace conference.

This is an objective account, only rarely somewhat nationalist in tone. Although some Greek materials are used, it is not definitive; French and Italian archives have not been consulted (nor, of course, Russian), and some published materials like Jäschke's articles have been overlooked. The documentation is usually copious, although for some important statements there are unfortunately no references. The book seems almost error-free. There are few revelations, though I was interested to learn that at Lausanne Lord Curzon had knowledge of the Turkish delegation's telegrams to and from Ankara, courtesy of British Intelligence. Much drama, and the color of personalities, might have been included were the book longer. It is, however, compact and well balanced—and now the best survey of Turkish diplomacy of the period.

RODERIC H. DAVISON
*George Washington University*

KEMAL H. KARPAT *et al. Turkey's Foreign Policy in Transition, 1950–1974*. (Social, Economic and Political Studies of the Middle East, volume 17.) Leiden: E. J. Brill. 1975. Pp. 233. 84 gls.

This is neither a systematic nor a definitive history of Turkish foreign policy in the period it covers. Moreover, it is subject to all the weaknesses of multiauthor works put together without disciplined integration and editing. While the authors are obviously well informed, only one contribution is documented, a serious drawback for historians who might wish to use this work in pursuing their own. Nevertheless, it is a book that serves very well in two respects. First, it provides an excellent summary account of Turkey's relations with the superpowers, with Greece and Cyprus, and with the Arab states and Israel. Second, it gives a good sense of the roots of Turkish foreign policy in geography, history, and nationalism, and of the links between foreign relations and the changes taking place in Turkish society and public opinion.

This second aspect comes through mainly in the contributions by Kemal Karpat, which make up almost half the book. He argues that Turkey's close alignment with the West in the 1945–1960 period had a heavy influence on internal developments, and then, without minimizing the effects of Cuba, Cyprus, and East-West detente, ties the shift in foreign policy in the '60s to what was happening at home—the fall of Menderes, the appearance of a new generation of intellectuals, discontent over maldistribution of new wealth, the attractions of socialist models of development, and other factors. The subject, however, is worth a good deal more exploration than it gets here. It emerges from the contributions of the other authors, as well as from Karpat's, that the turn toward a more independent foreign policy (which, incidentally, they all applaud) was the work not just of the intelligentsia and the new radicals but of Turkey's "establishment" also.

Except for the piece by George Harris on Turkey and the United States, a boiled-down version of his recent book on the subject, all the chapters are marked by a rather strong Turkish point of view. The discussion of the Cyprus question, for example, by Suat Bilge on the events up to 1972 and by Karpat on the crisis of 1974, should be read in conjunction with other accounts written by Greeks and by authors not identified with either side.

JOHN C. CAMPBELL
*Council on Foreign Relations*

## AFRICA

ROBERT W. JULY. *Precolonial Africa: An Economic and Social History*. New York: Charles Scribner's Sons. 1975. Pp. xii, 304. Cloth $12.50, paper $6.95.

*Precolonial Africa* is a description of the everyday economic activities of Africans at important points along the continent's historic path. This is a risky enterprise in the current state of knowledge, but Robert W. July's previous accomplishments give reason to hope a tour de force can be carried off. He is the author of *The Origins of Modern African Thought*, a fascinating account of West African intellectuals who were thinking and writing about the consequences of Africa's encounter with Europe from the late eighteenth century, and on through the nineteenth. His well-written text, *A History of the African People*, followed. A synthesis of Africa's early social and economic history, however, presents special problems as a result of the outpouring of research in the past fifteen years, and the only partially corrected emphasis historians have given to trade rather than production, and to state structures rather than village life.

In this text July has responded to the challenge by presenting concrete descriptions of economic activities and material culture. Two quite different kinds of description are set side by side: different in sources, historical method, and relation to major historical issues. In the first, July returns to central issues in recent historiography—the West African gold trade, the Atlantic slave trade, East Africa's coastal trade—in order to ask what the events meant to ordinary people, and what the activities looked like. The descriptions are solidly based on sources known to specialists, although

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often inaccessible to others. Finally a textbook tells what the shafts of a gold mine looked like, how traders used multiple currencies to strike a bargain, and how a camel caravan made its progress across the Sahara.

A second set of analyses unfortunately strays further from the sources and from secondary historical literature. July writes about farmers, herders, and hunters, in an attempt to fill in the incomplete historical picture of economic production. But the reconstructions are based largely on twentieth-century ethnography, filled in with fragments of documentary and archeological evidence. The resulting descriptions are often glaringly anachronistic. Preagricultural hunters are described as living in the desert, while quite clearly they had the choice of the richest parts of the continent before the expansion of agriculturists. The production techniques of modern Nupe are examined to illuminate the character of earlier farming in the savanna, even though Nupe's agriculture had already been modified by intense commercialization. July describes the difficulty of the African environment, in which diseases and pests multiply, but two of his major examples (swollen shoot disease and rinderpest) are drawn from the late nineteenth and twentieth centuries, under circumstances directly attributable to the colonial situation and its prelude. In most cases July gives clear dates for his examples, but the pastiche joins together bits and pieces from the many diverse eras, rather than elements which have persisted through the long term.

Two egregious errors, which are not central to the structure of the book, must be noted. July writes that "traditional Africa attributed all illnesses to supernatural causes. . . . [and] anatomy was overlooked and only symptoms treated," and that "art for art's sake . . . held no . . . meaning."

STEVEN FEIERMAN
*University of Wisconsin, Madison*

ELIAS TONIOLO and RICHARD HILL, editors. *The Opening of the Nile Basin: Writings by Members of the Catholic Mission to Central Africa on the Geography and Ethnography of the Sudan, 1842–1881*. New York: Barnes and Noble. 1975. Pp. x, 330. $26.50.

It is not commonly known that Central European missionaries traversed all but the last few hundred miles of the upper White Nile and explored much of what is now the Southern Sudan during the dozen years prior to the European discovery of the source of the White Nile by the Englishmen Speke and Grant. In presenting a sampling of the journals and correspondence of these missionaries, with an introduction that outlines the history of the Catholic Mission to Central Africa, Elias Toniolo and Richard Hill have brought to the attention of the general public the contribution of these men and the mission they served. The specialist in nineteenth-century Sudanese history also will benefit greatly from the publication of this material because there is a paucity of other sources relating to the Southern Sudan. Apart from the first-hand observations by the missionaries of the Dinka, the Bari, and other peoples, there is abundant reference in the texts to the antagonism that existed between the missionaries and the European traders and between the missionaries and the Turkish government.

The editors provide useful footnotes wherever the texts need clarification, and they preface each selection with biographical information about the author. Since most of the material is in excerpt form, they tell the reader where the original manuscript can be found. In this respect, the editors might have done well to point out that a modern German transcription from the original Gothic of the Knoblechar manuscript can be found in the Library of the Royal Commonwealth Society in London.

One wonders whether the last two chapters about relatively late journeys across sandy stretches of the Northern Sudan merit inclusion, especially if they appear at the expense of the complete Knoblechar journal. The title of the book, *The Opening of the Nile Basin,* suggests exploratory activity further to the south; so, if not the complete Knoblechar, then perhaps there might have been an excerpt from Father Kirchner, whose diary was not represented in the book, yet who was Pro-Vicar Apostolic of the Mission and whose photograph appears on the frontispiece.

Notwithstanding, the book is well edited, is readable both for layman and scholar, and is a most valuable addition to the field of African history.

J. W. FROST
*Indian Wells, California*

THOMAS S. COX. *Civil-Military Relations in Sierra Leone: A Case Study of African Soldiers in Politics*. Cambridge, Mass.: Harvard University Press. 1976. Pp. x, 271. $14.00.

Historians and social scientists alike should rejoice in the publication of Thomas Cox's study of civil-military relations in Sierra Leone. This first monograph about civil-military relations in this West African state is a timely and impressive work of interdisciplinary scholarship. It is an exemplary case study of military politics in Sierra Leone, an eminent contribution to our knowledge of African

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military praetorianism, and a valuable addition to the literature of comparative civil-military relations.

After reviewing the various theories of African military praetorianism and exposing their inadequacies, Cox provides a detailed reconstruction of the changing patterns of civil-military relations in Sierra Leone since independence in 1961. He argues that the period of military role continuity came to an end in 1966–67 when Sir Albert Margai, the prime minister, attempted to tribalize the army by recruiting and promoting his fellow Mende. Tribalization led to politicization and the formation of civilian-military cliques based on ethnic and regional bonds. The military elite disintegrated and military intervention soon followed. In short, the politicians intervened in the military, and the military in turn intervened in politics.

In sociological terms, the primary process of civil-military relations in Sierra Leone was cross-boundary clique formation among the political and military elite. Civilian-military cliques and patron-client relationships became the characteristic forms of elite interaction. The dual process of institutional boundary fragmentation and the formation of cross-boundary linkages was, therefore, the main focus of Cox's analysis. As he concluded, "The development of civilian-military cliques—united for purposes of mutual aggrandizement—provides the key to our understanding of the intermittent breakdown or threatened breakdown of civilian control of the Sierra Leone army" (p. 223).

Cox's study is based on a wide range of sources, including interviews with the principal military and political figures. His methodological approach combines both inductive historical analysis and a deductive application of the concepts, models, and propositions of civil-military relations theory to the Sierra Leone case. This is an authoritative work that is not likely to be superseded.

JOSEPH P. SMALDONE
*University of Maryland, College Park*

DONALD SIMPSON. *Dark Companions: The African Contribution to the European Exploration of East Africa.* New York: Barnes and Noble. 1975. Pp. xi, 228. $22.50.

"Chuma and my second headman Makatubu have worked like heroes, and I should indeed be but a poor mortal if I did not acknowledge the fact that the success of the Expedition has been to a large extent due to them," Joseph Thomson wrote after exploring the country between Dar es Salaam and Lake Nyasa during 1879 and 1880. Such praise for the African element in the European exploration of East Africa was not unwarranted, for a caravan's African personnel had as much bearing on an expedition's success or failure as did its European members. These "dark companions" have been called the most fundamentally important group of Africans in East Africa in the nineteenth century, because of their impact on local situations as well as for their vital function within the caravans.

Donald Simpson has attempted to trace the sometimes astonishing careers of the important individuals among the Africans who served as guides, interpreters, organizers, porters, hut-builders, cooks, nurses, and even pallbearers on the expeditions which opened East Africa to Europe. Often freed slaves who had been drawn away from their tribal environments, these Africans sometimes completed one journey only to enroll in another. The Yao Sidi Bombay, for example, traveled to the great lakes with John Hanning Speke, accompanied Henry Morton Stanley to find David Livingstone, and crossed the continent with Verney Lovett Cameron, covering perhaps 12,000 miles in all.

Although the author's intention to do justice to these African travelers is admirable, source material causes a problem. Except for a very few accounts by Africans, Simpson had to rely largely on the writings—some of them unpublished—of explorers and missionaries. He tends to echo their nineteenth-century sentiments, which he does not question. The result is only a paternal appreciation of the diverse personalities involved. The organization of the book is also Europe-centered with chapters based on the various expeditions. It might have been possible to organize the book around the outstanding personalities for whom sufficient sources exist, or around the roles of the various classes of personnel assigned to any expedition. However, by seeing in one volume even the basic facts about these African companions, the continuity of their efforts becomes apparent. The Who's Who of Africans which the author has compiled emphasizes this point. The maps are good, and the photographs interesting and well reproduced.

MARGUERITE YLVISAKER
*Boston, Massachusetts*

HAILE SELLASSIE I. *The Autobiography of Emperor Haile Sellassie I: "My Life and Ethiopia's Progress," 1892–1937.* Translated and annotated by EDWARD ULLENDORFF. New York: Oxford University Press, for the School of Oriental and African Studies, University of London. 1976. Pp. xxxii, 335. $13.75.

This is the first volume of the autobiography of Haile Selassie I, as apparently dictated in 1937 while the Ethiopian emperor lived in exile in Brit-

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ain following the Italian fascist conquest of his realm. It was first published in Amharic in 1973, and is now translated into appropriate English by Edward Ullendorff. Ullendorff's introduction and annotation are valuable in discussing the background of the book and in clarifying the many references to names, places, and events. His translation is good readable English, but he deliberately and carefully retains the special flavor of Ethiopia's quasi-divine monarchy and some of the syntactical intricacies of the Amharic original.

There is little new information in this imperial autobiography to add to the secondary sources on Ethiopia since Menelik or on the Italo-Ethiopian conflict. It is rather a lofty *apologia pro vita sua* in which the emperor, who dominated the country for over half a century, moves deftly through his rise to power as regent in 1916, his coronation as emperor in 1930, and his difficulties with the three European states whose colonial territories wholly surrounded Ethiopia. Much of the book is a translation of archival documents, speeches, and letters (some from the Amharic original) linked together by Haile Selassie's narrative and comment.

Nevertheless, it is an interesting book—proud in the emperor's stress on Ethiopia's long history and independence, fascinating in his fatherly advice to his troops about how to deal with the Italian airplanes most had never seen before, and poignant in his analysis of the growing fascist effort to subvert and finally to overrun his land. Above all, Haile Selassie emphasizes his determination to introduce "into the country western modes of civilization through which our people may attain a higher level" (p. 5). The first volume of the autobiography characterizes his opponents, who several times sought to overthrow him, as conservative and feudalist nobles opposed to necessary progress and change, which is ironic in that the ultimately successful effort to depose him after more than a half-century of his rule was led by a new generation of rebels that saw Haile Selassie as dragging his feet in the task of modernization.

BRICE HARRIS, JR.
*Occidental College*

## ASIA AND THE EAST

MAURICE MEISNER and RHOADS MURPHEY, editors. *The Mozartian Historian: Essays on the Works of Joseph R. Levenson.* Berkeley and Los Angeles: University of California Press. 1976. Pp. vii, 203. $10.00.

These essays that interpret and evaluate the work of the late Joseph R. Levenson, Sather Professor of History at the University of California, Berkeley, should be of interest to all intellectual historians and not to China specialists alone. While the central concern of his published work was the decay of the Confucian intellectual tradition and the development of a modern Chinese "mind," Levenson's historical concerns were very broad ones. The immediate topic of the brilliant trilogy, *Confucian China and its Modern Fate,* might be China, but in fact Levenson was engaged in the pursuit of a very sophisticated comparative intellectual history. This is evidenced by the tentative title of the work he left unfinished at the time of his tragic death in a boating accident in April 1969: *Provincialism and Cosmopolitanism: Chinese History and the Meaning of "Modern Times."* Levenson's penetrating treatment of the problem of "history" versus "value" in modern China, his important insights into the bureaucratic-monarchic tension that was at the heart of Confucian vitality, and his emphasis on the history of men thinking rather than the history of ideas per se, have had a major influence on the study of modern Chinese intellectual history in the United States. His colleagues and students discuss these and other aspects of his work, together with his life and methodology, in thirteen essays to which are appended a remarkable posthumously published piece by Levenson himself, "The Choice of Jewish Identity," a chronology, and a bibliography of Levenson's writings.

Five of the contributions strike me as particularly noteworthy, though I do not mean to derogate the rest in singling these out for attention in a brief review: John K. Fairbank's evocation of Levenson as graduate student and young teacher through excerpts from his correspondence about and with Levenson; Franz Schurmann's account of Levenson's intellectual development during his years at Berkeley; Benjamin I. Schwartz's "History and Culture in the Thought of Joseph Levenson"; Frederic Wakeman, Jr.'s tracing of the development of the theme of bureaucratic-monarchic tension; and James Cahill's "Style as Idea in Ming-Ch'ing Painting," which both modifies some of Levenson's views on the utilization of art as an index to intellectual currents and suggests a formidable program for the integration of the history of Chinese art and Chinese history.

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DUN J. LI, translator. *The Civilization of China: From the Formative Period to the Coming of the West.* New York: Charles Scribner's Sons. 1975. Pp. xi, 356. $12.50.

In the Chinese synoptic tradition from which it springs, this little pot-boiler would have been easily forgiven and quickly forgotten. In the context of

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western scholarship upon which it is foisted, it deserves only the latter fate.

One would hope to be gentle out of courtesy to a well-intentioned compiler who, the book jacket tells us, "has provided . . . the most important sources of Chinese history . . . , most of these . . . in English for the first time." So never mind that Dun J. Li himself is unaware (p. ix) that much if not most of his material has long existed in more accurate English translation. Never mind that the editions used for his translations are never cited, except in one ludicrous case when he cites the source of two documents of the year 1074 as a 1943 Chungking reprint of *A Collection of Essays* (?) (p. 208). Never mind multiple renderings of the same term, mispunctuated texts, abundant inaccuracies of translation, and generally uncritical acceptance at face value of these arbitrarily selected and thematically unrelated pieces. Consider only the compiler's own view of what history and its sources really are.

In a note to selection fifty, "Origin of the Hung-men Society," written by Li Dun-jen, the compiler of this work tells us: "This story, written by the author with information drawn primarily from *Chung-kuo pang-hui shih* [History of Chinese Secret Societies] (Hong Kong, n.d.) by Shuai Hsüeh-fu, should be read more as folklore than historical fact. It is written in such a way as to reflect the tenor of Chinese folklore which, as a child, this author used to hear in his native village" (p. 324). Thus there is actually Chinese civilization in this book. It is not, however, to be comprehended in the text, but must be apprehended in the mind of its compiler.

This is the quasi-historical folklore—feeling rather than fact—which the most ordinary Chinese know and love instead of history. That was a fine and lovable discrete tradition, and it perhaps explains Li's capricious and unsystematic selection of material. But decked out as scholarship, with a pretentious title and a $12.50 pricetag, it becomes an anachronistic piece of publisher's flummery.

CARL LEBAN
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DERK BODDE. *Festivals in Classical China: New Year and Other Annual Observances during the Han Dynasty, 206 B.C.–A.D. 220.* Princeton: Princeton University Press. 1975. Pp. xvi, 439. $17.50.

Nobody could be better qualified to write about the festivals of classical China than Derk Bodde. Early in his scholarly life, he translated *Annual Customs and Festivals in Peking* by Tun Li-ch'en (1936), and two years later he brought out *China's First Unifier*, a study of the same period with which he treats in his latest book. He had really only one predecessor, Marcel Granet, whose *Fêtes et Chansons Anciennes de la Chine* (1919) tried to reconstruct the festivals before the time which is studied by Bodde. There are, of course, early and recent Chinese studies of festivals of the Han period, but no comprehensive, analytic work exists. Bodde has utilized these studies and brought forth new data and new analyses; sometimes he discusses interesting questions which have only an indirect connection with the festivals, for example, the world view of the Han period and the problems of the Chinese calendar (pp. 26–41), or the nature of shamanism (p. 77ff).

Many questions still remain unanswered, but I agree with Bodde when he says "one becomes painfully aware of the limitations imposed by the sources" (p. 316). On a number of festivals, he has little to say, while the New Years' festival covers almost half of the book. One of the main problems is created by the complexity of the Chinese calendar. The Chinese had a solar calendar which was primarily used for astronomical calculations, but also some festivals were determined by the solar positions, and others were calculated by means of the solar position but with reference to the moon, similar to the way we calculate Easter. In addition, the Chinese calendars changed over time in the assignment of the day of New Year. Thus, the day of the *la* festival, called "People's New Year" by Bodde, is in some sources given as a year's end festival late in the twelfth month, in other sources as celebrated in the tenth month (p. 50ff). It is possible that this discrepancy is caused by the different calendar systems used by different sources, and complicated by attempts of later scholars to "correct" the discrepancy. The *la* festival has clearly something to do with the harvest, and the twelfth month, which now falls in January, is far too late to qualify for a true harvest festival. On the other hand, the festival is described as a true New Year's eve festival with a ritual expulsion of all the evils of the last year. But then, we have another New Year's festival of the day of the winter solstice which falls on December 22 according to the solar calendar. I cannot propose a solution more satisfactory than Bodde's.

One way, perhaps, to get further insight may be to compare two similar festivals, for instance the *la* festival with the *fu* festival which is half a year later than the *la*; both may have been popular festivals of the dead. Another possible approach would be to go back in time from well-described festivals still celebrated in recent times to poorly described festivals of classical China. Thus, the "pestilential influences" (p. 84) are still today driven into the water in Southeast and South China. The actions against those "who cling to the living" (p. 98) may

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not refer to the action against those who have been executed but more generally to all those whose souls had entered the body of another, still living person and thus made these persons obsessed, a belief that still is quite common. While some problems perhaps could be solved by this method, one of the main, unsolvable problems remains: "how much reality can we attach to descriptions of ceremonies, when these descriptions are found to be interlaced with stereotyped phrases or even clauses taken word-for-word from the passages in the ritualistic literature in which the ceremonies in question, or their prototypes, are initially recorded?" (p. 394).

The book will certainly stimulate much comment and further research, but will for a long time remain a standard work for anyone seeking an insight into the cultural and social life of the Han period.

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FREDERIC WAKEMAN, JR. and CAROLYN GRANT, editors. *Conflict and Control in Late Imperial China.* Berkeley and Los Angeles: University of California Press. 1976. Pp. xx, 328. $14.75.

This book resulted from the conference on local control and social protest during the Ch'ing period jointly sponsored by the University of California, Berkeley, and the American Council of Learned Societies in 1971. It covers the last four centuries of Chinese history, from the 1550s to the 1930s, a period that Frederic Wakeman interprets, from the vantage point of social history, as a coherent whole.

There are nine essays by eight authors. Wakeman alone contributed two essays: the Introduction, a long analysis of the gentry, and a case study of Chiang-yin's elites. Five authors use the familiar historical approach, whereas the others follow different methodologies. The article by Ira M. Lapidus is a comparative study of Chinese and Islamic societies; Fu-mei Chang Chen handles her topic from the legal viewpoint; and C. K. Yang analyzes mass actions in nineteenth-century China by quantification.

The essays by Wakeman, Jerry Dennerline, and James Polachek focus on the elites, especially the gentry. Chen's and Yang's articles emphasize the masses of people. Lapidus covers both the elites and the common people; Jonathan Spence sees Chinese society through drug addiction. Philip A. Kuhn's essay concerns China's local self-government. From different directions all the essays converge to the central theme: the social forces and local control; and some deserve special commendation. For instance, Wakeman's analysis of Chiang-yin's localism and loyalism is very forceful; Lapidus' comparative study is interesting; Chen's discussion of convicted thieves is effective; the essay by Spence shows thorough research; Yang's quantitative study is solid and inspiring; and Kuhn's article is thought-provoking.

Yet the book could be further improved. Its title could be reconsidered, because the last essay covers mainly the Republican period. Some statements seem challengeable. Dennerline maintains, for example, that the fraudulent trusteeship of land only benefited the landlords, not the members of the gentry whose privilege was usurped by the former (pp. 95–96). Perhaps this needs more documentation. There are several indirect citations (pp. 10–12, notes 26, 28–34), even though all the original sources concerned are accessible. The long quotation on pages 69–70 needs to be rechecked, and there are some misprints (e.g., pp. 67, 146, 24n, 238). As a whole, however, the book represents good scholarship and professional competence.

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JOHN FAIRBANK *et al.*, editors. *The I.G. in Peking: Letters of Robert Hart, Chinese Maritime Customs, 1868–1907,* in two volumes. With an introduction by L. K. LITTLE. Cambridge, Mass.: Harvard University Press. 1976. Pp. xxvi, 825, vi, 829–1625. $50.00 the set.

John Fairbank and his colleagues have added a new dimension to the study of modern Chinese history by meticulously editing this series of 1,437 letters of Sir Robert Hart, whom many consider the most influential Westerner in China during his time. These letters cover nearly forty years, from October 1868 to September 1907. In them, the editors have presented an important and hitherto unpublished primary source for the study of modern China, and have through copious footnotes and detailed indexing made it readily accessible. But their achievement is not limited to this alone: they have provided a model for the publication of the archival correspondence of other eminent Westerners (such as W. A. P. Martin) who lived and worked in China during the past 150 years.

In Sir Robert Hart, of course, they were dealing with a person of singular ability and importance. For forty-five years he served China as Inspector General of its Maritime Customs. These weekly or biweekly letters to James D. Campbell, who had served Hart in China as chief auditor and secretary from September 1866 to June 1868, and who was in charge of the London Office of the Inspectorate until his death in September 1907, pro-

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vide a close-up view of events in China from the standpoint of a very well-informed and intimately involved individual.

The "front matter" of the volumes includes a perceptive foreword, placing Hart's activity in the context of the treaty system; a history of the Hart-Campbell correspondence, of which Hart's original holograph letters are now in the library of the School of Oriental and African Studies, University of London (the telegrams which Hart sent to Campbell are not included); and a most informative introduction by L. K. Little, last of China's Inspectors General (1943–1950).

The Maritime Customs, over which Hart presided from 1863 until his retirement in 1908, was a truly international civil service that by 1895 employed some seven hundred Westerners and thirty-five hundred Chinese. Its operations went far beyond collecting customs duties, though in doing this it provided the most dependable source of revenue for the Chinese government during these years and later. Hart himself throughout his tenure acted as an unofficial adviser to officials of the Chinese government. It was under his guidance that the Service established the coastwise lights of China, completed the charting of the coast, developed the country's first national postal service, organized the display of Chinese goods and culture at important international exhibitions, instituted an admirable statistical system for the compilation of its trade reports, supported and helped develop a college for Chinese interpreters at Peking, and published monographs on a wide range of aspects of China's economic and social life. In short, it played an important role in China's efforts at modernization.

The letters show Hart at work. Many of them deal with the recruitment, examination, and selection of personnel for the Service, a matter to which Hart gave special attention. Many concern loans, interest and exchange rates, and other fiscal matters. Others deal with the purchase of everything from gunboats—a matter in which Campbell, under Hart's direction, acted as purchasing agent—to Hart's own clothes, and to violins and music, which, along with extensive reading, constituted his chief recreation. Many letters dwell on personal matters, such as Hart's concern for the education, health, and careers of members of his family. Above all, however, the letters show Hart's attitude toward China and the Chinese: his complete but not uncritical loyalty to the Chinese government he served; his insistence that customs employees learn the language and accept their position as "the *assistants—not the masters*—of the Chinese"; and his prophetic views—expressed in a series of essays written shortly after the Boxer Rebellion of 1900 and discussed in a number of these letters—on the ultimate triumph of Chinese nationalism and the end of foreign influence.

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MARK ELVIN and G. WILLIAM SKINNER, editors. *The Chinese City Between Two Worlds.* (Studies in Chinese Society.) Stanford: Stanford University Press. 1974. Pp. xiii, 458. $18.75.

*The Chinese City Between Two Worlds* is one of three volumes compiled from papers presented at conferences sponsored by the Subcommittee on Research on Chinese Society of Stanford University. The first volume is *The City in Communist China*, and the last is *The City in Late Imperial China*.

This work contains an introduction by Mark Elvin and a total of twelve disparate papers, eight dealing with mainland China and four with Taiwan. Among its contributors, insofar as I can identify them, are six socioanthropologists, four historians, two geographers, one demographer, and one political economist. The book apparently is a truly interdisciplinary effort, and as such it should be welcomed as a valuable addition to our knowledge of China.

Elvin in his introduction states that the purpose of this volume is to present systematic study of the process of modernization "between the two worlds of the old empire and the communist government of today." If so, these papers fall far short of the objective. The last three papers deal primarily with rural-urban migration in Taiwan and in Manchuria at the time of Japanese occupation and have very little relevance to this theme. In addition, it seems quite strained to include Winston Hsieh's paper on peasant insurrection in the Canton Delta, since it has very little to do with cities.

On the whole, one must say that most of these papers are detailed historical studies on certain aspects of cities, done in a scholarly fashion, with conclusions carefully drawn within the limits of evidence. One clear exception to this rule is Rhoads Murphey's, "The Treaty Ports and Chinese Modernization." It is a comprehensive, provocative, and controversial paper. Murphey draws from various sources of impressionistic information to argue that at the time of the arrival of European nations during mid-nineteenth century or earlier, China might have been politically and militarily weak, but economically had developed a vigorous system based on regional self-sufficiency. This system was quite effective to meet its own needs and to resist the importation of foreign manufactured goods. The treaty ports, though increased from five to sixty-nine, were indeed enclaves on the vast landmass of China, and the amount of trade carried on between these ports and China proper was so small and the contact between Chinese and

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foreigners through them so limited that it is sheer exaggeration to claim that these treaty ports contributed to China's modernization, let alone economic deterioration. In spite of Murphey's massive evidence, I feel that somehow the argument he tries to develop is less than conclusive.

The next important paper is Susan Mann Jones', "The Ningpo Pang and Financial Power in Shanghai." Jones traces the founding and development of the clique (*pang*) of merchants, natives of one prefecture, Ningpo. She then discusses how this small coterie made its fortune in Shanghai by operating native banks, how its members successfully adjusted themselves when foreign capital and modern banking were introduced, and how the clique rose to become an elite in financial circles and exerted their influence in that city.

Edward Rhoad's study on "Merchant Associations in Canton, 1895–1911," is another valuable one. Canton was both a provincial capital and one of the earliest treaty ports, and as such it attracted residents of both gentry and merchant classes. As usual, the merchants organized themselves into associations (or guilds) on the basis first of geographic origins and then of specific services. In competition with the officially sponsored organization, the general chamber of commerce, the merchants' guilds also founded an overall voluntary association for the purpose of overcoming parochialism and fostering mutual assistance. At the time of the 1911 Revolution the merchants decisively cast their destiny on the side of the Republic.

There are three more papers worth mentioning. Robert Kapp's, "Chung-king as a Center of Warlord Power, 1926–37," is concerned with the strategic location of Chungking and how the warlord Liu Hsiang took advantage of it to build up his financial and military power. David Buck's "Educational Modernization in Tsinan, 1899–37" is an account of the process of educational transformation from Confucian academia controlled by local gentry and teaching classics to the modern school system taught by professionally trained teachers in Shangtung Province with the capital city of Tsinan as a point of reference. Finally there is Stephen Feuchtwang's survey and analysis of the city temples in Taipei under three regimes. This paper deals with the transition of temples from the generally territorially or commercially based type to that of syncretistic belief, i.e. Taoist- or Buddhist-aligned. Both types can be regarded as reflecting the communal basis of society in Taipei.

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ANDREW J. NATHAN. *Peking Politics, 1918–1923: Factionalism and the Failure of Constitutionalism.* (Michigan Studies on China.) Berkeley and Los Angeles: University of California Press. 1976. Pp. xix, 299. $15.00.

As Andrew J. Nathan notes, the seventeen years following the 1911 Revolution constituted "perhaps the darkest corner of twentieth-century Chinese history" (p. 2). There have been relatively few publications on the period, mostly concentrating on the subjects of warlordism and revolution. There has been little discussion about Peking politics. For this reason alone, Andrew Nathan's book deserves special attention.

*Peking Politics, 1918–1923* is the product of careful and elaborate research. According to his impressive bibliography (pp. 263–77), the author cites more than 260 works in detailed footnotes. He has extensively used the materials of the British Foreign Office archives, which may have been supplemented by the diplomatic dispatches of the United States Department of State. (The omission of the latter source seems strange, since it is readily available on microfilm in American university libraries.)

An important asset of the book is Nathan's obvious grasp of modern research methods. He effectively combines both chronological and topical approaches in his analysis of the failure of constitutionalism in 1918–1923. The first three chapters provide a historical framework, which is substantiated by two skillfully chosen case studies in the second half of the volume. In an apparently convincing manner, the author argues that factional conflicts accounted for the lack of success of the constitutionalists in China. This is, however, at best an incomplete explanation.

It is perhaps impossible to illustrate an institutional failure without stressing the personal weaknesses of the leaders. I question Nathan's belief in the faith of Chinese politicians in constitutionalism. the republic had been "debased" long before Ts'ao K'un's accession to the presidency in 1923, and Yuan Shih-k'ai's monarchical restoration seven years earlier was doubtless a more degrading example of betrayal. While the militarists were in firm control of the Peking government, the constitutionalists offered an impotent challenge with no evident support from the masses.

Constitutionalism had never been a part of the Chinese political tradition. Without an adequate understanding of the Western democratic spirit, leaders in Peking turned to the commonly accepted avenue of factionalism. Ou-yang Hsiu of the Sung dynasty had considered the formation of factions an honorable practice for Confucian scholar-officials. Moreover, with the rise of Chinese Communism, many revolutionaries emphasized social changes as the only workable solution for China's problems.

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Under these conditions, constitutionalism really did not have a fair chance to take root.

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B. SHIRENDEV and M. SANJDORJ, editors. *History of the Mongolian People's Republic*. Volume 3, *The Contemporary Period*. Translated from the Mongolian and annotated by WILLIAM A. BROWN and URGUNGE ONON. Foreword by JOSEPH FLETCHER. (Harvard East Asian Monographs, volume 65.) Cambridge, Mass.: East Asian Research Center, Harvard University; distributed by Harvard University Press, Cambridge, Mass., 1976. Pp. xv, 897.

This ponderous book is the first substantive history of the Mongolian People's Republic (MPR), first written by native Mongols in their own language and subsequently translated into English. As such, it deals only with the history of the Republic (1921–1969) and not with earlier Mongol history. Most previous histories of this sort were by Mongols, but written first in Russian. This is a book by Mongols for Mongols, and therefore almost useless to all except a few outside the MPR until this translation. It is also the final volume of the official three-volume history of the Mongols from the Stone Age to 1969. The authors are the most prestigious Mongol historians of the Academy of Sciences of the Mongolian People's Republic, all with doctorates from Soviet universities and international reputations based upon appearances at orientalist congresses. With these qualifications and apparently unlimited access to the largely unplumbed state archives, they should have written a more useful book. The fault does not lie with the translators, William Brown and Urgunge Onon; a random check of portions of their translation shows it to be of the highest quality and extremely faithful to the style and syntax of the original.

The basic problem is that this work is not a history, either in the framework of the "Marxist-Leninist scientific approach" the authors seek to emulate, or in the context of western historiographical methodology. It is, instead, a compendium, often mind-numbing, of events and statistics, ranging from the amount of felt produced annually during the Third Five-Year Plan (400,000 meters), to the number of bombs dropped on the Japanese army on August 21, 1939 (86,000 kilograms).

This is not a text to be used in a classroom or as supplemental reading. One doubts that anyone will succeed in plodding from cover to cover through the endless wealth of data. It can only be used as a reference work, and then with caution; for example, sections on the destruction of the Buddhist Church in the 1930s and the 1939 war with Japan are extremely distorted. Nonetheless, the book is useful and a welcome addition for the specialist in Mongol history. The translators have added one hundred pages of endnotes that correct the more egregious errors and offer intrepretations of events that are often quite misrepresented by the authors, thus enhancing the usefulness of the work. The translators have also attached a bibliography of additional works on Mongolia, an extremely valuable glossary of Mongol terms, and—we can be thankful—an index without which the book would have had little utility.

One last question remains: Why has the Harvard University East Asian Research Center undertaken the printing of such a work? As I have noted, it is useful as a data book, and one must accept the statement in the preface that the Mongols should have the opportunity to speak for themselves in their own words. But in terms of cost and quality, one wonders if the Monograph Series is the place for that voice. Almost all that appears in these one thousand pages has been previously translated and made available by the Joint Publications Research Service or through various publications such as the *World Marxist Review*. Both of the latter series more accurately reflect the quality of this material.

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GARY D. ALLINSON. *Japanese Urbanism: Industry and Politics in Kariya, 1872–1972*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xiv, 276. $16.75.

Gary D. Allinson's primary concern in this work is the perennial question of convergence in the development of industrial societies—i.e. whether the social and political consequences of economic development are predictably uniform. Having chosen for his case study a city of 90,000 located near Nagoya in the highly urban and industrialized Aichi Prefecture, he traces in broad outlines the economic processes that transformed Kariya from a nondescript, isolated castletown of little national significance in nineteenth-century Japan into an important center for automobile manufacturing integrated into the nation's industrial system through the giant Toyota firm. Throughout Allinson attempts to demonstrate that the political consequences of this transformation have differed greatly from the experience of England or the United States.

Specifically, Allinson argues that except for a brief interlude in the decade following the Pacific War, Kariya politics have been remarkably free

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from the bitter clash of economic interests between corporate management and organized labor common in England, or the "bifurcation of power" between local leaders and company representatives familiar to comparable American communities. Instead, the Toyota firm has dominated Kariya politics since the 1920s, and its industrial workers have accepted that domination with marked docility. The major change has been in the tactics Toyota has used to maintain its influence. Initially in the 1920s and 1930s Toyota exercised its political power through local officeholders drawn from the old-line families among the preindustrial elite. When the gradual erosion of community solidarity and the upsurge of left-wing activism in the troubled postwar years threatened Toyota dominance, the management of the firm responded by openly confronting the more progressive factions in local politics and by the late 1950s successfully reasserted its influence. In Allinson's view the key to that success was the support of labor union leaders and the rank and file of Toyota workers.

In search of explanations for the configurations of community power in Kariya and the divergences from the American experience, Allinson turns to an examination of the organizational structure of the industrial workplace and the "heritage of social values," which he conceptualizes as a "dynamic phenomenon" with "persistent . . . but not *static* features as 'traditional values' tend to have" (p. 256). Much of what he finds is not totally unexpected to the reader familiar with the literature on Japanese industrial relations, but Allinson does provide fresh data on the interrelations between economic change and political behavior by skillfully weaving together population statistics and election returns with detailed material on the history of the Toyota enterprise and biographies of local political figures drawn from numerous sources, including personal interviews.

Obviously, this is a work of interest to an audience wider than those who specialize on Japan. Yet the book will leave many of the latter less than fully satisfied. Allinson has clearly bitten off too much in attempting to digest a full century of local history in less than three hundred pages, and the result inevitably falls short of the "more holistic" social history that he himself advocates so articulately. His too-brief treatment of the first half of his time span—the fifty years between 1872 and 1922—gives particular cause for dissatisfaction on this score. Because he focuses on political conflict defined largely in terms of changes in officeholding and stresses industrial development as the primary socioeconomic variable, one suspects that he may well have overstated both the degree of social continuity and of political consensus in these early decades, which he characterizes as a period of "smooth transition from agrarianism to industrialism" (p. 44). On the one hand, we are told too little about the impact on social life of tax reform, new legal codes, compulsory education, military conscription, and the other instances of the attempt by the central government to mobilize and integrate the local populace into the nation state not to suspect that more was happening in Kariya than Allinson describes. Nor are these developments irrelevant to the "heritage of social values" which Allinson sees as a dynamic factor underlying the political quietism of the generation of workers in the 1920s and 1930s. If we are to understand those social values more fully, we need from the social historian more concrete information about such matters within the specific localities where future Toyota workers were socialized. On the other hand, although Allinson's data on the continuity of local office-holding are impressive, one wonders if Kariya was really so atypical as to have escaped so completely the intraelite conflict that others report was true of local politics elsewhere in the Meiji period.

Specialists in the period following 1922, to which Allinson devotes much more space, will have other quarrels over what has been left out. Still, it should be stressed that Allinson's monograph is the first of its kind on a Japanese urban community to appear in English and, unlike most local Japanese historians, the author has framed analytical questions, attempted systematic answers, and placed the whole within an explicitly comparative perspective. Despite our quarrels, it is a long step in a very fruitful direction.

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J. C. MASSELOS. *Towards Nationalism: Group Affiliations and the Politics of Public Associations in Nineteenth-Century Western India*. Bombay: Popular Prakashan. 1974. Pp. xi, 332.

J. C. Masselos provides a fascinating account of the slow growth of public life and political institutions in Bombay and Poona from the 1840s to the 1880s. Much has been written about early nationalism in India and the modern institutions that gave it shape, but Masselos' study is one of very few to focus upon the ungainly and heterogeneous city of Bombay. Unlike Calcutta, or even Poona, which is introduced largely to point up the distinctive character of the presidency capital, Bombay had no one dominant community. Rather, diverse linguistic, religious, and caste groupings, each with business as well as professional elites, jostled for dominance within it.

In the first chapter—in many ways the best section of the book—Masselos lays out the social

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geography of the city, and argues that the public life of Bombay was shaped by the conflict between these "encapsulated" communities, with their inward-looking loyalties, and the "integrative" status and occupational groups that grew up as the city developed. The subsequent chapters, decade by decade, detail the process by which first merchants and then the educated of Young Bombay, through such open bodies as the Bombay Association, came to play a part in civic affairs. The process was neither a simple nor an easy one. Conflicts of interest often drove *shetia* (businessmen) and educated apart, while the pull of community remained always strong. Badruddin Tyabji especially found himself torn between the demands of his sectarian and his professional constituencies.

Some may find Masselos' analytic categories too rigid and his conclusion too abrupt, for he does not clearly delineate the presumed supersession of civic by national concerns in the mid-1880s. He might too have taken into account such existing writing on the subject as that of Christine Dobbin. But the author's sensitive understanding of the Bombay scene makes this a most useful addition to the literature on nineteenth-century Indian politics.

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DAVID R. STURTEVANT. *Popular Uprisings in the Philippines, 1840–1940.* Ithaca, N.Y.: Cornell University Press. 1976. Pp. 317. $17.50.

Applying the ideas of Redfield, Hobsbawm, and Lanternari with skill and sophistication to modern Philippine history, David R. Sturtevant has written what must become the standard work on popular unrest in the Islands since the mid-nineteenth century. In this excellent study, he has sympathetically analyzed and felicitously described the frustrations of what Joseph R. Hayden called the "unrepresented minority" of Filipinos.

The book begins with three chapters on "The Sources of Stress" that contain, among other delights, the best concise explanation anywhere of the social structure of rural society in the modern Philippines. This is followed by a marvelously insightful restatement of the paradoxical nature of American colonialism, which by its emphasis on what Sturtevant rightly terms "political fundamentalism" at the expense of any recognition of the harsh socioeconomic disparities of Philippine life ultimately compounded the terrible disparity between rich and poor. "Peasants were given a tantalizing glimpse of freedom and abandoned to find their own way out of the agrarian wilderness" (p. 55).

The remaining ten chapters of the book deal with the various millenarian and secular upheavals that have erupted violently throughout the Philippines in the past century. Utilizing a variety of carefully selected original sources and writing with great verve, Sturtevant recounts in chronoligcal order the fascinating phenomena of the Cofradía de San José, the Guardia de Honor, the Babailanes, the Pulajanes, the Salvadoristas, Colorumism, the Entrencheradistas, the Kapisananes, the Ricartistas, and the Sakdalistas. The villains of this tragic rollcall of suppressed rebellions are uniform in Sturtevant's eyes: caciquism, Catholicism, and colonialism. "Inadvertently, the United States strengthened the social legacy of Spain: elitists derived augmented powers; peasants inherited mounting squalor" (p. 59).

This volume is not only an important contribution to Philippine history, but is also the kind of book of which Southeast Asian historians generally can be extremely proud. Because of the superb way in which Sturtevant handles the important subject matter and because of the clarity and forcefulness of his writing, this is an ideal example of a "microsomic" work that has truly "macrocosmic" significance. Historians generically—Americanists, Europeanists, Africanists, Asianists, or whatever—can pleasurably read, easily understand, and immeasurably profit from Sturtevant's study of popular uprisings in the Philippines. Moreover, Cornell University Press has done an outstanding printing job and has provided a most helpful series of maps throughout the text. I fervently hope that the publisher will recognize the remarkable quality of this work and will promote it vigorously beyond the specialist community.

GRANT K. GOODMAN
*University of Kansas*

S. L. VAN DER WAL, editor. *Officiële bescheiden betreffende de Nederlands-Indonesische betrekkingen, 1945–1950* [Official Documents Concerning Netherlands-Indonesian Relations, 1945–1950]. Volume 4, *31 maart–16 juli 1946*; volume 5, *16 juli–28 okt.* 1946. (Rijks Geschiekundige Publicatiën, Minor Series, numbers 39 and 41). The Hague: Martinus Nijhoff. 1974; 1975. Pp. xxvi, 724; xxiii, 727.

These two volumes are valuable additions to an important publication launched only a few years ago. For students of the Netherlands-Indonesian conflict all volumes are fascinating and indispensible reading. Most of the documents are drawn from the Department of Overseas Territories in The Hague. Furthermore, there are minutes of pertinent Cabinet meetings and the Coun-

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cil of Military Affairs and documents on British, Australian, and American policies or views.

Of course, one does not find in this mass of documents the entire story of Dutch-Indonesian relations; for instance, there is nothing on the political developments and public opinion in the Netherlands. A large number of documents are the correspondence between Lieutenant-Governor-General Van Mook and the ministers of overseas territories, J. H. A. Logemann and J. A. Jonkman. These papers reveal, as did those in the preceeding volumes, Van Mook's frustration over the lack of Dutch understanding of Indonesian political realities. If the statesmen and politicians in the Netherlands finally accepted some of these realities—including Sukarno—this change in policy was largely the result of Van Mook's efforts.

One of Van Mook's major achievements was the decision by The Hague to meet with Republican delegates at the Hoge Veluwe in April 1946, an event amply documented in Volume Four. It has been alleged that Van Mook tried to "stump" the Dutch government by previously signing a preliminary agreement with the Republic that The Hague could not reject. If this is true, the documents do not reveal such a ploy. The conference failed, as Van Mook pointed out, because the Dutch were too much concerned with "*clausureeringen*," their inability to make a "generous gesture," and "strict formalism" (4: 369, 483). Of course, neither side really had a mandate to make a "generous gesture." The Dutch government did not have an elected, democratic base, while the Republican delegation, as the documents show, did not even have the support of its own government.

In September the new Catholic-Labor government appointed a special three-man commission-general to assist in the negotiations with the Republic. There is a bit of irony in this: although Van Mook had been pleading for several months for the appointment of someone to assist him in his enormous task, he considered this commission "completely unacceptable" (4: 633) and threatened to resign. As we learn here, the creation of this commission was part of a domestic political agreement. But was this commission also a political device to curb Van Mook's powers? If so, the documents do not tell.

They do reveal that the commission functioned well and that cooperation among the members and with Van Mook was excellent. It is interesting to observe how quickly the three men took a more advanced position than their government. They might have been influenced by the report of the so-called Koets mission to Djokjakarta, one of the most interesting documents in Volume Five. Contrary to what had been alleged about the Republic, this Koets report was most favorable. Unfortunately, it never seems to have affected most Dutch politicians and public opinion.

The documents also may shed further light on the true intention of the Netherlands government. While most Dutch officials realized they could not return to the prewar days, there was a determination, as Jonkman stated privately, that Holland "must remain a world empire" (5: 59, 2n). Van Mook tried to implement that policy at the so-called Malino Conference that is extensively documented here. At Malino delegates from Indonesia, except Java and Sumatra, agreed to establish a federation and to continue cooperation with the Netherlands. Critics of the conference lambasted Van Mook's policy, however, which, they asserted, was one of divide-and-rule designed to retain Dutch influence.

How serious the Dutch were about their intentions is revealed by their frantic military preparations. They succeeded in transporting some ninety-one thousand troops, including many draftees, to Indonesia before the British finally extricated themselves from their delicate task by the end of November. As was the case during our Vietnam adventure, many of the draftees refused to go, and in September 1946 some twenty percent had failed to report back for duty at embarkation time.

Volume Five ends with the resumption of negotiations in the fall of 1946, resulting in the conclusion of a truce agreement and the first suggestion of a Netherlands-Indonesian Union. Ironically, neither one elicited an enthusiastic response from The Hague.

GERLOF D. HOMAN
*Illinois State University*

J. W. DAVIDSON. *Peter Dillon of Vanikoro: Chevalier of the South Seas*. Edited by O. H. K. SPATE. New York: Oxford University Press. 1975. Pp. xix, 351.

In this case the ability of the biographer far exceeds the achievements of his subject, Peter Dillon. Dillon possibly rates article-length treatment for his search for and discovery in 1827 of the fate of the French explorer, La Perouse, who had disappeared in the western Pacific in March 1788. A trader and ship captain, he discovered by a combination of shrewd deduction and a good general knowledge of that part of the Pacific, that Vanikoro Island, one of the Santa Cruz group, was the final resting place of La Perouse.

Even though the world accepted Dillon's proof (he was awarded a medal plus a small monetary stipend by the French government) he expected far more. The remainder of his life was spent fruitlessly proposing various exploration and settlement schemes, and chasing well-known British

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and French political and naval men in a vain search for a rewarding commission and greater recognition.

The author, the late J. W. Davidson, was Professor of Pacific History at the Australian National University until his death in 1973. He was considered one of the leading scholars of island histories in the western Pacific. His more than twenty years' labor-of-love attachment to Dillon resulted in this volume, which was finished by an associate, a sad reminder to this reviewer of another New Zealander, the late J. C. Beaglehole who, similarly attached to a subject, found a most worthy one in Captain James Cook.

One can only agree with Davidson's conclusion regarding Dillon, that although he had "great gifts of courage and vitality" he "was not a 'great man' . . . and his life as a whole was a failure" (p. 308).

W. P. STRAUSS
*Oakland University*

FRANK K. CROWLEY, editor. *A New History of Australia*. Melbourne: William Heinemann; distributed by Holmes and Meier Publishers, New York. 1975. Pp. xii, 639. $26.00.

MARILYN LAKE. *A Divided Society: Tasmania During World War I*. Melbourne: Melbourne University Press. 1975. Pp. x, 213. $20.00.

It is a great shame that this first book has not been reviewed in these pages before now, for it merits the close and early attention of all serious historians. *A New History of Australia* is an example the best and most useful kind of one-volume national history.

Frank K. Crowley asserts in his preface that during the past two decades "the study of Australian History has attracted some of the best scholars in the humanities," and surely he is right. In this collection we have the mature and bold work of twelve historians, including Crowley's own, covering the years from 1788 to 1972 with a passion, diversity, and originality that will be envied by all who feel history should be both instructive and stimulating. The difficult trick, which the editor has ably demonstrated, is to gather authors who are willing to challenge dullness, orthodoxy, and traditional assumptions and methods, who are willing to thrust forward some interpretations of their own, and who are capable of doing and respecting the latest research in their fields—and do so with style. This is not to imply that all of these essays have been written by young upstarts and radicals. Such is not the case, for all of these authors are respected scholars—and quite possibly a few of them may even be conservatives; but the work is fresh and the editor, by finding representatives of the several flourishing historical schools in Australia, has brought about a pleasant tension. Crowley has added to the book's appeal by encouraging his contributors to consciously consider the interests and needs of many students and other readers who want to see the links between Australia's contemporary situation and the recent and not-so-recent past.

All criticisms of a work of this caliber are quibbles, but I have two of them: first, there are no maps; second, the reader deserves more information about the authors. As partial compensation, the book has an excellent and complete bibliography.

The author of *A Divided Society* maintains that the war caused the disintegration and fragmentation of Tasmania rather than its unification, as other scholars have argued. As she puts it, "Class was set against class, creed against creed, district against district, soldier against civilian" (p. 189). Some of the issues that foreshadowed this conclusion were these: the paradoxical nature of Australian and Tasmanian nationalism within the framework of the British Empire; racism; the temperance movement and nativism; the role of women; religious sectarianism; the various dilemmas of the Labor movement (being in power, having serious reservations about the war and capitalism, their attempts to stay in power and protect the rights of their Catholic and alien supporters, etc.); conscription; the role of women; and the Irish and Russian revolutions. Altogether this is a perceptive, well-written, and useful contribution to our understanding of the reaction which followed World War I.

F. P. KING
*University of Denver*

D. G. E. HALL. *Henry Burney: A Political Biography*. London: Oxford University Press, for the School of Oriental and African Studies, University of London. 1974. Pp. xv, 330. $16.00.

D. G. E. Hall has written a succinct study of the diplomatic career of Henry Burney, the government of India's envoy to Siam from 1825 to 1827 and British resident at the Court of Ava from 1829 to 1838. Burney, scion of a distinguished intellectual family, left no record of his private life, and Hall has had to recreate the man from official dispatches and diaries. What emerges is a person of strong will, moral certitude, humor, tact, and sensitivity. As a prototype of the scholar-diplomat, Burney mastered Burmese and Thai and wrote copiously on the histories, customs, arts, and literatures of Siam and Burma. As an "insider" he was able to assume the role of teacher as well as student and to instruct his hosts in current western

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notions of international diplomacy and "civilized" behavior.

In dealing with the Siamese Burney performed the delicate task of calming their fears of British military intentions and simultaneously convincing them that his government would not tolerate their attempt to dominate the states of the Malay peninsula. The lessons he gave them in the realities of international politics were one reason the Siamese were able to preserve their independence in the latter nineteenth century.

While the Siamese were skittish, the Burmese were sullen and resentful of their recent defeat by the East India Company. It was Burney's responsibility to teach them the folly of reneging on the humiliating Anglo-Burmese treaty, and he had to do so with minimal support from Calcutta, obsessed at the time with India's northwest frontier.

Hall stays close to his sources and has written a detailed—perhaps over-detailed—narrative of Burney's activities, enriched by the author's authoritative judgments and scholarly evaluations of Burney's decisions. A glossary and charts illustrating governmental organization in Siam and Burma would have helped guide the reader through the complexities of domestic politics. The book's chief appeal to the nonspecialist is as a record of the initial volatile contacts between a Western power and two proud, secluded Asian kingdoms when each side probed and tested the other to learn its purposes and the limits of its patience and fortitude.

BLAIR B. KLING
*University of Illinois, Urbana-Champaign*

E. DANIEL POTTS and ANNETTE POTTS. *Young America and Australian Gold: Americans and the Gold Rush of the 1850s*. St. Lucia: University of Queensland Press. 1974. Pp. 299. $20.75.

This volume is packed with informative material, much of it from primary sources, and the footnotes are especially instructive. It seems safe to say that all future scholars in this field will find the work of great practical importance. Unfortunately, it is published in ten-point type in unleaded lines, a print which makes the pages annoying to read.

The book opens with concise biographies of the United States consuls to Australia from 1836 to 1863. Another chapter describes the mining techniques brought by Americans to Australia. Others list Americans who, during the gold rush, served as carpenters, stonemasons, lawyers, and sellers of books and of patent medicines. The experiences of immigrant doctors in Australia and in California during the rush are carefully compared. Americans operated some of the best Australian hotels. American barkeepers introduced popular drinks, and American Negroes served as excellent chefs. Attempts to import California lynch law proved less successful.

The authors describe, with revealing details, the stagecoaches and steamboats imported and built by Americans in Australia. A splendid chapter portrays the thespians who visited the Australian diggings. We are told also about the American circuses and minstrel shows which "misrepresented the American Negro in much the same way that English drama treated the stage Irishman."

Every year Australians celebrate their battle for independence at the Eureka Stockade, the first shots of which were fired by the Independent Californian Rangers. The authors conclude, however, that American immigrants "had only a small impact on the development of politics in the Australian colonies." Far more important, they say, was the example of the Americans' successful revolution against British colonialism.

JAY MONAGHAN
*University of California, Santa Barbara*

KEITH AMOS. *The New Guard Movement, 1931–1935*. Melbourne: Melbourne University Press; distributed by International Scholarly Book Services, Beaverton, Ore. 1976. Pp. xii, 142. $15.00.

Keith Amos has produced an interesting though by no means definitive account of an organization that came very close to usurping the government of the state of New South Wales in the dark days of the 1930s Depression. Known as the New Guard, this militaristic, quasi-revolutionary, right-wing movement claimed over a hundred thousand members by the end of 1931 and enjoyed the support of several hundred thousand Australians.

Under the leadership of a blimpish lawyer and former army officer, Colonel Cambell, the New Guard armed itself and trembled on the very brink of outright revolution. Plans for a coup were drawn up that involved kidnapping Jack Lang, the Labour Premier of New South Wales, together with his entire cabinet. Police reports to the government indicate that a coup might well have succeeded had New Guard leaders possessed the nerve to go ahead. Luckily for Australia, the irresolution of the New Guard leadership meant that the country was spared the specter of civil strife and bloodshed so common then in the towns and cities of Europe, as right- and left-wing anarchists fought to control the streets.

Amos indulges in considerable semantic legerdemain to prove that the New Guard at the height of its powers was a fascist organization; it is here that

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the real weakness of the book lies. He makes some breathtaking assertions on the flimsiest of evidence; possibilities become probabilities and then established facts in the space of a few lines. The author has succumbed to the temptation to label a movement of which he disapproves as fascist. He does so despite clear evidence that the further the Gilbertian Cambell inclined toward a recognizably fascist outlook, the more rapid became the decline of the movement, as its members clearly preferred to leave it rather than accept an Australian variant on the European fascist theme. By August 1933 the New Guard movement had virtually ceased to exist, sure evidence of a massive rejection of fascist ideology by conservative Australians.

F. C. CLARKE
*Macquarie University*

## UNITED STATES

RAIMONDO LURAGHI. *Gli Stati Uniti*. (Nuova storia universale dei popoli e delle civiltà, volume 16.) Turin: Unione Tipografico-Editrice Torinese. 1974. Pp. xxiii, 747. L. 17,500.

Raimondo Luraghi is a remarkable scholar who became a historian relatively late in life after adventures as an antifascist partisan and a career as a prize-winning journalist. He came to American history relatively late, too, after producing scholarly works on the antifascist resistance in Piedmont, the economics of Risorgimento, and the interplay of European colonialism and nationalism. Luraghi's *Storia della guerra civile americana* (1966), perhaps the best one-volume study of the American Civil War, is now in its fourth edition. Unfortunately Luraghi's article, "The Civil War and the Modernization of American Society" (*Civil War History*, 1972), is his only work written in English.

*Gli Stati Uniti* is part of a prestigious Italian series on world history, and publication of the book confirms Luraghi as Italy's premier historian of the American experience. The work has no peer and is sure to become the standard study of the United States written in Italian. Although *Gli Stati Uniti* is a general study, the American Historical Association gave the author a special bicentennial award in connection with its competition among monographic contributions to United States scholarship written in a language other than English.

The work is narrative history; it will appeal to a general audience and will serve as a textbook for Italian students. Yet *Gli Stati Uniti* is also richly analytical and displays interpretative themes informed by a highly sophisticated Marxism and an emphasis on comparative history. Luraghi emphasizes the diversity of contributions to the American experience. As might be expected he pays considerable attention to the "new immigrants" of the late nineteenth and early twentieth centuries, many of whom were Italians. But he also devotes much space and attention to the influence of Indian, Spanish, French, African, and Southern subcultures. In so doing he de-emphasizes the contributions, if the significance, of traditional American "Yankees."

Luraghi translates economic issues and class interests into ideology and perceives ideological assumptions as prime movers in the American past. He emphasizes a persistent tension between the ideal of freedom and the interests of the bourgeois class—a tension which he suggests characterizes the American world view.

As is often the case in bold summaries, specialists will certainly quarrel with Luraghi's analysis of their particular field of expertise. For example, his treatment of Reconstruction is, by implication at least, a bit "soft" on the South. Yet a work so masterful in analysis and so broad in scope can stand upon its merits. *Gli Stati Uniti* is an outstanding contribution, not only to American studies in Italy, but also to American studies in America.

EMORY M. THOMAS
*University of Georgia*

FRANCIS JENNINGS. *The Invasion of America: Indians, Colonialism, and the Cant of Conquest*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture. 1975. Pp. xvii, 369. $14.95.

*The Invasion of America* is a powerful assault on the racist mythology that has so long obscured an honest view of Indian-European relations in early America. We embrace the mythology even as we speak of "settlement" or "wilderness" in those crucial years. Indian peoples experienced *an invasion*, and it was a homeland, not empty acreage, that the invaders seized. Francis Jennings ably explores the many ways that Europeans masked their exploitative maneuvers in pious, self-serving phraseology, deceiving not only themselves but generations of historians to follow. "The myths created by the cant of conquest endure in many forms," writes the author. In this brilliant two-part book, he endeavors to destroy as many of those fictions as possible.

Jennings succeeds admirably. In part one he examines Atlantic Coast tribes in the light of recent scholarship. He sorts myth from reality in ten topical chapters ranging from precontact population estimates to forest diplomacy. Especially

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important chapters concern tribal governments, trade, agriculture, and warfare. Part one offers a theoretical base that can be applied to other contact situations. It is superb ethnohistory worthy of comparison with Edward Spicer's *Cycles of Conquest*.

In this reviewer's opinion, part one is weakened by the decision to omit any extended discussion of Indian religion. In choosing to limit his remarks to "European conceptions," Jennings has neglected to examine the dynamic role that Indian religion played in the contact situation. Clearly, Indian theological ideas deserve more scholarly attention.

Part two examines Indian-European relations in New England from the earliest encounters to the end of the 1670s. It launches a blistering attack on the Puritans and their latter-day apologists. Jennings portrays the Puritans as grasping, land-hungry hypocrites—despicable opportunists who routinely engaged in wanton acts of slaughter and treachery to achieve their ends. Accordingly, his analysis of the Pequot War and King Philip's War stands in stark contrast to recent studies by Leach and Vaughan. Jennings' interpretation is persuasive, and his version of events will stand. But Jennings' people are often strangely one-dimensional. Exploitative Englishmen, innocent Indians—at times old stereotypes reappear in reverse. The ethnohistorical balance of part one collapses occasionally in part two.

Sometimes strident, frequently wry, and often sarcastic, this outstanding book is bound to produce both illumination and controversy. The historiography of Indian-European relations will never be the same.

JAMES P. RONDA
*Youngstown State University*

DOROTHY JEAN RAY. *The Eskimos of Bering Strait, 1650–1898*. Seattle: University of Washington Press. 1976. Pp. xvi, 305. $14.95.

This fundamental work, admirably conceived and researched, and well written, provides a cultural history of the Bering Strait area of Alaska from the initial white contact about 1650 until the gold rush of 1898 swamped the native population with newcomers. The author bases her work on an exhaustive study of printed sources, clarified by nearly twenty years of ethnographic field work.

After a short geographic survey, she examines information on Alaska received in Siberia in the seventeenth and early eighteenth centuries, garbled and fragmentary, but providing the earliest record of life on the Alaskan side of the Bering Sea. On the basis of geographic conditions she doubts the voyage of Dezhnev around the East Cape of Siberia, which may reopen a question which Soviet historians believe settled. There is more reliable information for the period from 1732 to 1779, including names and approximate locations of a number of native settlements. Rumors persisted, however, until the middle of the nineteenth century of Russians established in Alaska in the seventeenth or early eighteenth centuries. The author gives good reasons for rejecting these reports. More reliable information follows from the expeditions of Cook and Billings, Kotzebue, Avinov, Etholen, Khromchenko, Shishmarev and Vasiliev, Beechey, the searchers for Sir John Franklin, and the accounts of members of the Western Union Telegraph Expedition. She then examines the changes under American jurisdiction, the impact of whaling and mining, of schools and missions, and the effort to introduce reindeer herding. Here good use is made of interviews; seventy-five native informants are listed in an appendix.

The Bering Strait region was once popularly considered to have little culture and no history; this book indicates that there is an abundance of both, with many possibilities for further research in literature—especially Russian sources—and in the field. The book is well designed, with good maps, many illustrations, a list of sources, and a detailed index.

RICHARD A. PIERCE
*Queen's University, Ontario*

JOHN T. SCHLEBECKER. *Whereby We Thrive: A History of American Farming, 1607–1972*. Ames: Iowa State University Press. 1975. Pp. x, 342. $12.95.

Considering the strong agrarian roots of America, serious writing on the subject of agricultural history has not kept pace with that of other topical fields. Each year brings one or two volumes on selected agricultural topics or regions, yet there has been dire need of a synthesis—a volume that would wrap it all up in a neat package. This book is an attempt to do just that, and it comes close. Beginning with land acquisition in the Jamestown Colony in 1607 and concluding with comments on topics as diverse as anhydrous ammonia, 2,4-D, and Tolstoy's historical determinism, John T. Schlebecker covers the major events in the history of American agriculture with remarkable thoroughness and evenness. Moreover, he delves into peripheral topics as they impinge upon the major theme. With sections dealing with mercantilism, transportation, marketing, genetics, conservation, governmental programs, and consumer behavior, the author keeps his subject in the larger national perspective, an approach that places the book in a category by itself.

Arranged chronologically into five major periods

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(1607–1783, 1783–1861, 1861–1914, 1914–1945, and 1945–1972), the book's twenty-nine chapters provide a framework for the discussion of an astonishing number of topics. Land acquisition commands a great deal of attention with most of four chapters devoted to methods used by colonial, state, and federal governments. Technology also receives considerable space with several chapters devoted to the development of tools, tractors, and machinery.

As might be expected from such a comprehensive volume, much detail, especially documentation, is lacking. There are no references except for direct quotations, and conflicting interpretations are omitted. It is arranged as a textbook with brief subsections, a format that permits wide coverage of topics, but at considerable sacrifice in cohesion and readability.

The book's virtue lies in its comprehensiveness and breadth of view, yet virtue to some may be sin to others. One feels a recurring sense of frustration at seeing certain topics handled so fleetingly. For example, hybrid corn receives a little over one page, meat-type hogs get one paragraph, and artificial insemination is given sixteen lines. The twentieth-century "miracle crop," the soybean, is virtually ignored. Furthermore, the agricultural novice is apt to be puzzled by lack of detailed description. How many nonfarmers would even recognize an "endgate spreader" or "power take-off" or "side-delivery rake," much less understand their function?

Despite such objections, the book is a good one, written by one who knows the subject well. It will be welcomed, particularly by those seeking a comprehensive view of the subject. In that sense it far surpasses anything previously available.

SAM B. HILLIARD
*Louisiana State University*

ROBERT L. CHURCH and MICHAEL W. SEDLAK. *Education in the United States: An Interpretive History.* New York: The Free Press. 1976. Pp. x, 489. $12.95.

The major theme of this book is "the varying commitment among Americans to 'mass schooling' and equality of education." On the one hand, this has involved the expansion of opportunities to go to school and a broadening of responsibilities placed on the schools and, on the other hand, the limiting of those opportunities on the basis of race and social class. In chapters on antebellum district schools, colleges, and academies, the common school movement, educational reform in the South, vocational education, the university, progressivism in education, and equality of educational opportunity, Robert L. Church and his coauthor Michael Sedlak explore such issues as the *ad hoc* pattern of schooling and the blurring of college and secondary education during the nineteenth century, the bureaucratization of public schooling, the social reform and social control goals of educational reformers, the use of schooling to retain social status, the development of special education for blacks, the poor, and the university-bound, and changing definitions of equality of educational opportunity.

While followers of recent trends in the writing of American educational history will discover little new material here, Church and Sedlak have done a commendable job in synthesizing that literature and providing a broad social and political context for the history of schooling. In exploring the ambiguities and twistings of the educational past, they have given us the best one-volume survey of American school history currently available. The bibliography is good, the choice of topics pertinent, especially the discussions of secondary education and the uses of schooling to delineate social status, and the writing clear and thoughtful.

Having recommended the book highly, however, let me note some of its deficiencies. As in most textbooks, the writing tends toward the bland, and lacks the bite of monographs by Katz, Kaestle, Lazerson, Spring, Tyack, and Troen. By excluding the Colonial period from their survey, Church and Sedlak have bypassed much exciting and innovative research, especially in the history of the family and literacy. Much of the discussion of people of color is presented from the perspective of white social reformers. There is also a too frequent resort to categories that limit rather than expand insight, e.g. in the frequent labeling of educational positions as either Whig or Democrat during the first half of the nineteenth century. Finally, one would have wished for more about the relationships of schooling to economic development, social structure, and mobility, and a somewhat greater willingness to take chances in the conclusions. There is some of this, but not as much as one would like.

Still, for those who want a solid survey of the field for use in social and cultural history courses, *Education in the United States* is an excellent choice.

MARVIN LAZERSON
*University of British Columbia*

PHILIP S. HAFFENDEN. *New England in the English Nation, 1689–1713.* New York: Oxford University Press. 1974. Pp. xiii, 326. $18.50.

The scholarly monograph may be expected either to present unfamiliar data or to recast familiar facts in a new interpretative mold. That this book does neither is not wholly the author's fault. His work was completed in 1971. Three years passed

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before it was published. In the interim, several works appeared which pre-empted much of Philip S. Haffenden's contribution. For example, his opening chapter, "Revolution and Settlement in the Colonies, 1689–1692," has been overwhelmed by D. S. Lovejoy's like-titled and now standard work, issued in 1972. In concluding that the Glorious Revolution "thrust" the people of Massachusetts "into an empire they had refused earlier to recognize," and so forced them to defend their colony's diminished stature in the English world in terms of a religious mission no longer appropriate to that world, Lovejoy epitomizes the message of Haffenden's work. Of course, this message is well-known to both these authors, and to every student of the period, as the burden of Perry Miller's *The New England Mind: From Colony to Province*. Frankly premised on previously written conceptual pieces and overtaken by recent, detailed works explicating them, Haffenden's discussion seems to lack the qualities of imagination and initiative which constitute "originality" in scholarship.

In addition to a destructive publication delay, *New England in the English Nation* suffers from a lack of editing. At least consistency in the spelling of proper names might have been imposed. Still better would have been clarification of both concepts and prose.

The potential for clarification in Haffenden's work is realized in his crisp and direct presentation of Canada's relations with her French metropolis and her New English neighbors. Adapted from his chapter in the *New Cambridge Modern History*, this account compares colonial status and self-definition in Canada and Massachusetts. In continuing the modification of Parkman's classic comparison, it is one of this volume's two particular contributions.

The other is its time span. Recent studies of New England and of its relation to the mother country have tended to neglect or even omit the quarter century of Atlantic war and imperial integration that followed the revolutionary settlement of 1692. Haffenden's narrative supports a conclusion that this era of military defense against the French empire and social accommodation to the English empire may have been as important to the formation of New English attitudes as the preceding age of provincial autonomy had been.

To construct this narrative, Haffenden depends largely on the *Calendar of State Papers, Colonial*. Thus his work does not convey either the nuance of expression or the wealth of illustration required to successfully delineate its subject, the "sophisticated relationship with the parent state" supposedly sought by Massachusetts men. This relationship was neither "formalized nor acknowledged by informal courtesies, save in the uncertain months following the revolution of 1688. That there was no real partnership was evident by 1711, if not earlier." Perhaps the essential difficulty of this work is that its subject never existed.

STEPHEN SAUNDERS WEBB
*Syracuse University*

KENNETH P. BAILEY. *Christopher Gist: Colonial Frontiersman, Explorer, and Indian Agent*. Hamden, Conn.: Archon Books. 1976. Pp. 264. $15.00.

Christopher Gist passes briefly through the pages of American history during the period 1748–1759, when he functioned as frontier trader, explorer for the Ohio Company, scout, soldier, and ultimately Deputy Agent of Indian Affairs under Edmund Atkin. A sound biographical study of Gist would be useful, but unfortunately Kenneth P. Bailey's attempt to fill that need falls far short of the mark.

The account is developed mainly in narrative form. Evidence to sustain the narrative sometimes is sparse, the organization occasionally breeds confusion, and interest is not sustained. There is little new to excite the scholar, and much that is far too shopworn for modern customers. Indians and frontiersmen are the familiar stereotypes of earlier days, unrefined by recent scholarship. "Haughty" Braddock is still the contemptible old redcoat of long-discarded schoolbooks. In the bibliography one notes with regret that some important recent works of scholarship which could have been helpful to the author have not been utilized, among them the indispensable volumes of L. H. Gipson.

Gist himself never emerges as a living human being, although Bailey certainly tries to wring the utmost from the sources available. In the main, Gist appears as reflected in the writings of other men, such as Washington, who knew him. The author has not been able to resist the temptation, particularly strong for those attempting biographies of tertiary figures, to exaggerate the importance of his subject. Finally, the book contains several factual errors and a deplorable number of misspellings, faulty transcriptions, and irregularities and inaccuracies in citation.

DOUGLAS EDWARD LEACH
*Vanderbilt University*

ROBERT W. LOVETT, editor. *Documents from the Harvard University Archives, 1638–1750*. Part 4, *Documents, 1638–1722*; part 5, *Documents, 1722–1750*. (Publications of the Colonial Society of Massachusetts, Collections, volumes 49 and 50.) Boston: the Society. 1975. Pp. xxix, 399; xiii, 403–855.

In 1925 the Colonial Society of Massachusetts published the first two volumes of *Harvard College Re-*

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*cords* (*Colls CSM*, 15 and 16), edited by Albert Matthews, and in 1935, added a third (volume 31), edited by several persons. These latest volumes are thus the fourth and fifth issued by the society devoted to the history of Harvard before 1750. They include only documents from the Harvard Archives, but what we are given is a very generous selection from several series in the archives. Robert W. Lovett provides a helpful preface, which explains much about the archives and the selections from them; and in an appendix, John M. Hoffman reconstructs the history of College Books I-III, which were published in *CSM*, 15. These accounts are extremely valuable to anyone using the five volumes in print, for the records they contain have a history almost as complicated as that of early Harvard. It is worth noting that Lovett finds helpful the list of the main record series (to 1800) in the Harvard archives in Samuel Eliot Morison's *Harvard College in the Seventeenth Century*, though it is thirty years old.

The documents that constitute volumes four and five are not of much use for the study of the intellectual life of the college, though something of the spirit of presidents and tutors can be made out. There is virtually nothing about curriculum, though there is much on the library and its holdings, and there is an interesting document on the Dudley Lectures. And scattered throughout are documents which give partial glimpses of the lives of the college's faculty and its students. The "Disorder Papers, 1676" reveal that Harvard students enjoyed raising hell with young males and females from Cambridge, many of them servants. Rum "mixt with water and sugar," and cider, presumably sufficiently hard to soften inhibitions, produced conduct that shocked Thomas Danforth, the college steward and a Cambridge magistrate. The worst of it seems to have been singing and dancing into the early hours of the morning. The servants absorbed most of the blame—they had drawn the students from their studies—and they received the punishment.

Far more of the documents deal with the governance and finance of the college. There is a fascinating set of responses from Massachusetts towns in the 1650s to an appeal from the college for funds. These statements indicate that respect for learning pervaded the colony, along with regard for a learned clergy. They also speak eloquently of how hard life was in these communities. More revealing of the inside of college finance is the Dunster-Glover case, amply represented by petitions and other papers. Increase Mather's attempts to secure a charter late in the seventeenth century find expression in several documents. Mather seems to have spent more of his time worrying himself—and others—about his salary or, when that subject tired him, trying to avoid living in Cambridge.

Governance remained a leading concern of the college in the eighteenth century, as the documents from the fellowship controversy of the 1720s testify. Tutor Nicholas Sever took the lead in this struggle, arguing that the tutors should serve on the corporation. His essays are among the most interesting in these volumes.

The largest group of letters is from Thomas Hollis, Sr., and deal with his gifts to the college. These letters, of course, are well known and have often been used by historians interested in eighteenth-century dissent as well as in Harvard. For the student of Harvard's history, their chief value may be their information about the library. The editor's attempts to identify authors and titles are especially helpful.

Indeed, the editing of these volumes is excellent. Its value is nowhere clearer than in the last major group of documents on the lands given Harvard before 1750. The full history of gifts of land cannot be reconstructed from these documents, but like so much else in these volumes, what appears is highly suggestive, and well presented.

ROBERT MIDDLEKAUFF
*University of California, Berkeley*

DAVID C. HUMPHREY. *From King's College to Columbia, 1746–1800*. New York: Columbia University Press. 1976. Pp. x, 413. $15.00.

Here, for the first time, the eighteenth-century origins of Columbia University are given an extended scholarly examination. David C. Humphrey's study makes a contribution to recent historical literature that corrects and modifies generalizations about American culture that derive too exclusively from New England and Puritan experience. He develops a convincing sense of what going to college in New York in the decades before the American Revolution was like—its tone, its texture, the ways in which King's differed from other colonial colleges. The result is a notable achievement: educational history that enriches our understanding of the politics, economics, and social patterns of the age.

On one level this is an account of how religious diversity and an urban environment frustrated efforts to make King's and, after the Revolution, Columbia a prototypical American college, denominational and residential in nature. The chartering of King's as an Anglican college proved impossible in pluralistic New York; room for dissenting clergy was found on the governing board. After the Revolution the maintenance of the residential pattern of student dormitories collided with official priorities which placed a higher value on intellectual resources than on student resi-

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dence: Columbia became a college for students who lived off campus. In its interdenominational character and its rejection of dormitories, the college was less typical of its contemporaries than it was of the directions in which American society was moving.

On another level Humphrey has succeeded, especially through the cautious but imaginative use of group biographies of students and college governors, in drawing a picture of an institution designed to serve the needs of an economic and social elite. King's was an instrument of the advantaged, especially of the advantaged professional and merchant classes of New York who were attached to concepts of hierarchical order in society. Half of King's alumni were loyalists; its alumni in the ranks of American patriots were conservative friends of an ordered society—Alexander Hamilton, John Jay, Gouverneur Morris, and Robert R. Livingston. Certain chapters—"Higher Education from an Elitist Perspective," "The Education of Wise and Good Men," and "The Undergraduate Experience"—are model case studies in educational history.

FREDERICK RUDOLPH
*Williams College*

JAY J. WAGONER. *Early Arizona: Prehistory to Civil War*. Tucson: University of Arizona Press. 1976. Pp. xvi, 547. $14.95.

Writing the history of any state is a peculiar experience. Many of the secondary sources have been produced by amateur historians—and thus are suspect—because professional historians consider working on local topics beneath their dignity. However, almost every state has a history so long and colorful that all the original sources cannot physically be consulted by one person in a lifetime. Finally, the professional historian who does such a study learns that his work usually is too erudite for the public, while his colleagues in history departments do not acknowledge his accomplishment as genuine scholarship.

Such has been the situation in Arizona. Few professional historians there have done anything about the state in which they live, leaving the work mostly to journalists and folklorists. Jay J. Wagoner has demonstrated that the writing of his state's history can be done, that materials are available, that the story is exciting, and that the task is worthwhile.

This book contains, as the title implies, the history of Arizona to the Civil War, and is a companion to Wagoner's earlier work, *Arizona Territory, 1863-1912*. There are errors in it, especially in the era of the Mexican War, and it does little to set Arizona's history in national and international context. In total, however, Wagoner is to be congratulated for an excellent piece of scholarship presented in readable fashion.

ODIE B. FAULK
*Oklahoma State University*

KENNETH COLEMAN. *Colonial Georgia: A History*. (A History of the American Colonies in Thirteen Volumes.) New York: Charles Scribner's Sons. 1976. Pp. xvii, 331. $15.00.

This is a worthy addition to the Scribner's series, History of the American Colonies. Kenneth Coleman, a specialist in eighteenth-century Georgia studies, has organized his material in a traditional fashion, but each chapter is a comprehensive statement based upon the best recent scholarship. The description of the first voyage is a moving account. The story of the settlement of the various ethnic groups is the best part of the book. The myth about debtors is dispelled; more important was the fact that one-eighth of the settlers had had no agricultural experience. The central theme is well stated on page 110: "The Trustees never understood that human self-development is important and cannot be directed and molded from without."

Two directions for additional study are opened up by this work. One can now examine the history of the province as part of the total story of the southeast, that is in relation to South Carolina to the north and Florida to the south. The expansion of South Carolina into Georgia is crucial, as Coleman himself admits that the Carolinians "would have been in many ways the best colonists." William Wragg's dismissal from the South Carolina royal council was paralleled by Clement Martin's dismissal from the Georgia council. Governor James Wright admitted that the Savannah Sons of Liberty were influenced from Charleston. What was the connection between the desire of the parish of St. Johns to become part of South Carolina and William Henry Drayton's proposal for a confederation?

The second direction demands an analysis of groups. The author himself would have benefited from this in as much as he states that one will never know the names of the Georgia Sons of Liberty, yet later asserts that eighteen of twenty-five persons elected to the assembly in 1768 were "avowed Sons of Liberty." More important is the need to probe Governor Wright's support in the province, depicted in only general terms on pages 270–71. This support might have been analyzed by newspaper research such as that suggested on page 216. Then the story of the coming of the Revolution would not have been simply Wright versus the Patriots. Indeed the admirable description of the ethnic groups in the beginning of the volume would have been a good base from which

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to examine the mechanic and merchant classes. Local history can be pushed that far. But if one is to launch in new directions, this work is the indispensable starting point.

GEORGE C. ROGERS, JR.
*University of South Carolina*

ROBERT V. HAYNES. *The Natchez District and the American Revolution*. Jackson: University Press of Mississippi. 1976. Pp. viii, 191. $6.95.

The title of this work is misleading, because Robert V. Haynes' study is not so much an account of the Natchez District as it is a treatment of the confused hostilities on the Mississippi River involving loyalists, patriots, Spaniards, and Indians. To be sure, Haynes has not ignored Natchez, the most northerly settlement in British West Florida. He describes how the British occupied Fort Panmure (old French Fort Rosalie), how immigrants, many of them persecuted loyalists, arrived and exploited the district's rich soil, how surprised inhabitants pledged their neutrality during the Willing raid of 1778, how, when Spain besieged and conquered Baton Rouge in 1779, Natchez also was included in the capitulation, and how in 1781 the citizens revolted and for a fleeting moment overturned Spanish authority.

But often Haynes, who has read widely in manuscript and secondary sources, tells us more about developments at Baton Rouge, Manchac, New Orleans, or even Pensacola, and Mobile. One map does not even show Natchez, and the portraits he includes of William Dunbar and Elias Durnford are of men who had almost nothing to do with Natchez during the war. Though little new appears in this study, it is a perceptive synthesis of military operations in the lower Mississippi Valley.

Too many aspects of Natchez's wartime role, however, still remain obscure. Such topics as life under Spanish rule before 1783, political, social, and economic development, the status of women and Negroes, the effects of wartime immigration, distinctions between rural and town dwellers in the district, or even the district's bounds are largely ignored. It is to be hoped that Haynes in time will extend his research into local court records and other sources dealing specifically with Natchez and disclose much more about the early development of this remarkable frontier settlement.

J. LEITCH WRIGHT, JR.
*Florida State University*

CHARLES W. TOTH, editor. *The American Revolution and the West Indies*. (Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1975. Pp. xi, 225. $12.95.

While there is need for a book analyzing the changing relations between the thirteen colonies/states and the Caribbean islands during the era of the American Revolution, this collection of essays does little to fill the gap. Charles W. Toth has assembled nineteen selections from books and articles, most of which turn out to be either obsolete or trivial in character. The majority of pieces reprinted here were written before 1940; the essay on Cuba by James M. Callahan dates back to 1899 and was occasioned by the Spanish-American war. With Charles M. Andrews, Frank Wesley Pitman, and Claude H. Van Tyne setting the prerevolutionary scene, and Lowell J. Ragatz and Wilbur H. Siebert presenting the postrevolutionary situation, the book has a decidedly dated framework. Richard Pares is the only recent economic historian included. Surprisingly few of Toth's authors have done any archival research in the islands.

Toth concentrates on trade and diplomacy and pays no attention to social issues such as slavery, even though the islanders heavily influenced mainland slaveholding patterns in the eighteenth century, and the American Revolution considerably influenced slavery in the islands. This book is compiled without reference to the ideas of Philip Curtin, David Brion Davis, Eugene Genovese, Elsa Goveia, Winthrop Jordan, Orlando Patterson, Richard Sheridan, Eric Williams, or any of the other leading current workers in the field. Toth is more of a story-teller than an analyst, and several of his selections, such as Michael Craton's account of the revolutionary war in the Bahamas, are lively and entertaining. But most of the essays do not read very well, perhaps because they are so severely abridged; nor do they hang together very well, despite Toth's running editorial commentary. There is a lot of unnecessary repetition: for example, John Adams is quoted four times as saying that molasses was an essential ingredient in American independence (pp. 31, 36, 54, 135). On the whole, it is hard to see why the Kennikat Press issued this book, or who they think will read it.

RICHARD S. DUNN
*University of Pennsylvania*

PAUL DAVID NELSON. *General Horatio Gates: A Biography*. Baton Rouge: Louisiana State University Press. 1976. Pp. xiii, 319. $17.50.

This competent but pedestrian biography will supersede Samuel W. Patterson's study, but only because the latter book was not a very good one. Paul David Nelson's biography tells us a bit more

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about Gates' ancestry, places his "connections" and promotions within the British army in a more meaningful context, and provides a better explanation for his rapid rise in the ranks in England despite his relatively low social and economic background. The book recounts the familiar outline of Gates' military career in America: his differences with Washington, the Gates-Schuyler controversy for command of the Northern Department, his victory at Saratoga, the contrasting defeat at Camden, Gates' involvement in the so-called Conway Cabal, and his participation in the Newburgh Conspiracy. Generally speaking, the book must be judged a failure on two counts: it adds little to what we already know, and it does not provide an analysis of Gates as a man or the motivation behind his clashes with his colleagues.

The main reason we learn little new is that no fresh meaningful manuscript material was uncovered by Nelson. Forced to go over much of the same ground covered by Patterson, Nelson came to many similar conclusions. Patterson, however, was sloppy in his footnoting; Nelson was more exacting in this regard, and scholars no doubt will cite his study. Insofar as the rehabilitation of Gates' reputation is concerned, that task was accomplished years ago by Patterson and by Bernhard Knollenberg in his *Washington and the Revolution, a Reappraisal: Gates, Conway and the Continental Congress*.

The distinguishing characteristic of Gates' military career was the frequency of his fights with fellow officers. Since he disagreed with the commander-in-chief, Gates became the "whipping boy" of the Revolution because so many scholars are and were Washington idolators. What this study lacks is any analysis to provide an explanation for Gates' actions. Nelson's hypothesis of intense personal ambition is too simplistic and never emerges as an overriding theme. Instead of old-fashioned narrative history, we need an approach employing the techniques of comparative history or a behavioral psychological study to tell us why this warrior of the Old World continued his contentious ways in the New.

GEORGE ATHAN BILLIAS
*Clark University*

WILLIAM E. NELSON. *Americanization of the Common Law: The Impact of Legal Change on Massachusetts Society, 1760–1830*. (Studies in Legal History.) Cambridge, Mass.: Harvard University Press. 1975. Pp. ix, 269. $14.00.

William E. Nelson argues that before the Revolution the legal system in Massachusetts reflected and promoted a stable, unified society based on shared ethical values, in which government lacked coercive power and local communities determined the substance of the law. The criminal law protected religious and moral values, while the law of property maintained social unity and stability, serving community needs first rather than individual convenience. After the Revolution ethical and religious unity was destroyed; Massachusetts society came to consist of aggressive, materialistic individuals and competing interest groups; and the law became a tool by which dominant groups imposed their will on the rest of the society.

The strength of Nelson's book lies in its careful analysis of specific developments such as changing debtor-creditor relations and the emergence of tort and contract law. Its weakness lies in the schematic nature of the argument: the conclusions Nelson draws about Massachusetts society from his legal data are too sweeping and categorical to be convincing.

A related difficulty concerns the explanation of changes in law and society. Nelson reasons that the American Revolution was an event of such transcendent importance that it must have affected the course of American law; by his own admission he tried to interpret the data so as to portray his sense of the social change that must have accompanied legal change. In effect, Nelson has invoked Bernard Bailyn's "transforming radicalism" of the Revolution as a self-evident and sufficient explanation of social and legal change. To apply this suggestive idea, however, does not explain satisfactorily the actual process of change. There is finally the problem of the relationship between law and society. Although Nelson's title announces a concern with the impact of legal change on Massachusetts society, his work tells of the impact of social change on the law. He holds that the law reflected changes in thought and society. One wonders, however, whether or in what sense the author thinks he has described the difference that law made in the history of Massachusetts society. To answer that kind of question is perhaps the more challenging task of the legal historian.

HERMAN BELZ
*University of Maryland, College Park*

MAXWELL BLOOMFIELD. *American Lawyers in a Changing Society, 1776–1876*. (Studies in Legal History.) Cambridge, Mass.: Harvard University Press. 1976. Pp. ix, 397. $15.00.

Despite the title of this book, it is not a comprehensive history of the American legal profession, nor a study of the relationship of the development of the legal profession to parallel changes in the United States during its first century. That caveat aside, it is a valuable collection of individual studies of

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American lawyers. Bloomfield offers nine studies of significant American lawyers, four of which have been previously published in scholarly journals and are here presented in slightly rewritten form.

This collection is a mixed one, but all the studies touch in some way upon the development of the American bar. Five of the chapters are among the best examples of legal biography published in recent years. So many examples of this genre are self-serving eulogies of the recently deceased that they are of little value to the legal historian, but Bloomfield's work is measured, well balanced, and precise. He has chosen his subjects well, and each of them is of interest in his own right, as well as being illustrative of a major development in the law.

Two of these biographical sketches are of contemporaries of quite contrasting ideological positions. Peter Van Schaack of New York was exiled to England for six years under New York's Banishing Act of 1778, while William Sampson came to the United States in 1806 in exile for conspiracy to establish an Irish republic. Van Schaack's is a poignant account of the difficulties under which opponents of the Revolution lived. Sampson, an early civil-rights lawyer, was to spearhead the codification movement in the law.

In later chapters Bloomfield gives accounts of Frederick Grimké, lawyer and political theorist whose writings on democratic institutions have been long neglected; of William Pitt Ballinger, Texas lawyer who served as a Confederate receiver of alien (i.e. Union) property; and John Mercer Langston, first black Ohio attorney and founder of the Howard University law school.

The second group of articles deals with a disparate series of topics—"The Family in Antebellum Law," "Upgrading the Professional Image," "Riot Control in Philadelphia," and "Antilawyer Sentiment in the Early Republic." In general, the quality of the articles is high, and the book is an excellent addition to Harvard University Press's Studies in Legal History. One looks in vain, however, for a statement of Bloomfield's own conclusions on some questions. He seems, for example, to challenge the general view of legal historians, notably Roscoe Pound and Anton-Hermann Chroust, that democratizing patterns in the period 1830–1870 caused a notable decline in the professionalism of the bar, yet he does little to evaluate and critique their studies. Nevertheless, this collection provides some valuable, and fascinating, background for American legal history in the first century of the Republic.

NORBERT BROCKMAN
*Bergamo Center*

WILLIAM M. FOWLER, JR. *Rebels Under Sail: The American Navy during the Revolution.* New York: Charles Scribner's Sons. 1976. Pp. xi, 356. $15.00.

All too frequently historians of the American Revolution have ignored the maritime aspects of the conflict, or at best have reflected slight understanding of that decisive element. William M. Fowler, Jr. addresses himself to the subject in a study which contains a wealth of information and is a welcome addition.

The Continental Navy, the seagoing force created by Congress, is the focal point of this book. The author broadens his central theme to treat two army adventures on the water, namely General Washington's schooner squadron during the 1775–76 siege of Boston, and Benedict Arnold's fleet on Lake Champlain. Other significant facets of the American war effort at sea—privateering and state navies—are but touched upon.

In addition to selected naval operations, the scope of this work includes the vicissitudes of naval administration by Congressional committee and the building, outfitting and manning of ships. The particular topical arrangement selected to present the material does the author a severe disservice. The reader arrives at the demise of the Continental Navy, with the sale of the last vessel, *Alliance*, in 1785, long before Fowler's narrative takes the Navy to sea or even builds the ships. As a result, we have a series of disjointed parts which never come together to form a whole picture which would give substance to the author's valid thesis that the American Revolution was a naval war.

Extensive and good use is made of the *Naval Documents of the American Revolution* volumes being published by the U.S. Department of the Navy, and other recent scholarship. The bibliography is exhaustive and excellent. Fowler has ample grist for the mill, but unfortunately it is not always ground fine enough. Factual errors in the book, while not fatal, are damaging. The author takes the Baltimore-built continental frigate *Virginia*, under Captain James Nicholson, on a voyage to Martinique carrying tobacco and dispatches. Actually *Virginia* was captured in Chesapeake Bay on her maiden voyage before she ever got to sea. Again, Nicholas Biddle is praised as the capable and aggressive commander of sloop-of-war *Saratoga*. Biddle never saw the *Saratoga*. He was killed in action while commanding frigate *Randolph* before *Saratoga* was taken into the Continental Navy and placed under command of Captain John Young. Perhaps pressure to have the book on the shelf for the 1976 Bicentennial year accounts for these errors in an otherwise finely researched effort.

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Fowler has an easy and colorful writing style. He can be read with profit by all with an interest in the naval history of the American Revolution. However, do not discard your vintage copies of Charles Oscar Paullin's *The Navy of the American Revolution* (1906) and Gardner Allen's *A Naval History of the American Revolution* (1913).

WILLIAM JAMES MORGAN
*U.S. Department of the Navy*

JOHN F. KASSON. *Civilizing the Machine: Technology and Republican Values in America, 1776–1900.* New York: Grossman Publishers. 1976. Pp. xiv, 274. $15.00.

*Civilizing the Machine* belongs to the genre of American Studies associated with Henry Nash Smith, R. W. B. Lewis, John William Ward, and, especially, Leo Marx. In the opening essay on "The Emergence of Republican Technology," as well as here and there throughout the volume, Kasson recapitulates Marx's theme of the impact of technology on America's "pastoral values" as revealed in the writings of Jefferson, Emerson, and other literary intellectuals and in other artistic forms, such as that by-now-hackneyed depiction of the railroad as part of a pastoral setting in George Inness' *The Lackawanna Valley*. Kasson's four additional chapters consider: Lowell, Mass., as an episode in cultural history; "Emerson's internal dialogue on the impact of technology upon the American imagination"; various "aesthetic responses to and claims for machinery"; and an examination of four "utopian and dystopian novels of the 1880s and '90s," namely Bellamy's *Looking Backward,* Twain's *Connecticut Yankee,* Donnelly's *Caesar's Column*, and Howells' *Traveler from Altruria.*

Among these five essays, perhaps the most interesting and original is "The Aesthetics of Machinery," especially in its discussion of reasons for the elaborate ornamentation of American machines that was to be so severely criticized by Lewis Mumford and other twentieth-century historians of technology. On the whole, however, students of American cultural history will find little that is new to them in this volume. The essays are readable and intelligently presented along lines laid down by earlier scholarship.

What remains highly questionable is the author's claim that the whole of the volume somehow equals more than the sum of its five essays; that, taken together, these essays serve to "show . . . the way in which American technological development and republican values profoundly shaped one another." In fact, *Civilizing the Machine* is simply a collection of tenuously related essays, touching upon a theme far broader in scope than these essays collectively suggest it to be.

GILMAN M. OSTRANDER
*University of Waterloo, Ontario*

WILLIAM R. BROCK. *The United States, 1789–1890.* (The Sources of History: Studies in the Uses of Historical Evidence.) Ithaca: Cornell University Press. 1975. Pp. 352. $15.00.

This is the first volume of the Studies in the Uses of Historical Evidence to deal with sources for United States history. Written by one of Britain's leading historians of nineteenth-century America, the book is not only a study of historical sources bearing on the first hundred years under the Constitution, but also an admittedly personal statement about American history. William R. Brock organizes his discussion around four major areas: 1.) the growth of the nation, dealing primarily with statistical records and the relationship of the Civil War to growth and change; 2.) the government of the United States, concentrating on the records of all branches of the national government and those who held positions in it; 3.) the life of the people, focusing on the print media, farmers, townspeople, blacks, immigrants, and laborers; and 4.) a speculative section on the American mind, considering the role of religion, education, ideas, and particularly the concept of nationalism. He intentionally excludes two central elements which characterized the century—the westward movement and the growth of business.

*The United States: 1789–1890* is a series of essays which should be required reading for the graduate student beginning work in nineteenth-century history, or for a specialist in a particular topic of the period who needs familiarity with allied sources outside his specialty. While the author concentrates on written sources and ranges among local and national records including published and unpublished documents, he is eclectic rather than exhaustive, illustrative rather than definitive. In that selectivity, however, he has surveyed the nineteenth-century landscape, noting gaps left in historical research, illustrating the uses and abuses of some sources, and warning against the temptation to draw broader generalizations than the available data warrant.

In reviewing available material, he is at his best in discussions of the central role of the Civil War, the national records bearing on the development of the country, and the government and laws shaping, and being shaped by, that growth. The section on the life of the people is more derivative, however, relying heavily on the work of Frank Luther

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Mott for journalism, Paul W. Gates for farming, Brinley Thomas and Charlotte Erickson for immigration, and Richard C. Wade for slavery. In the final section, that dealing with the American mind, Brock discusses the material which American historians use to analyze the process of change and concludes with the observation that they are too introspective and need to examine the American experience in a comparative framework and from a broader perspective.

The volume's style is unpretentious and readable, and the content is valuable for anyone wishing to investigate nineteenth-century America through available primary sources. It is therefore appropriate both for the classroom and for the library.

NICHOLAS C. BURCKEL
*University of Wisconsin—Parkside*

JAMES R. GIBSON. *Imperial Russia in Frontier America: The Changing Geography of Supply of Russian America, 1784–1867*. (The Andrew H. Clark Series in the Historical Geography of North America.) New York: Oxford University Press. 1976. Pp. x, 257. Cloth $10.00, paper $6.00.

The history of empire has been, almost always, an uneven tale: high adventure and daring, privation and the destruction of truly grand and noble dreams, profit for some, and loss for many others. Russian America was persistently on the loss side of the ledger, and did not even compensate with salubrious climate or richly cultured natives.

One of the main reasons for failure (albeit the principal long-range one was the feverish wasting of the only valuable resource, fur-bearing animals) was the extreme difficulty of provisioning the outposts built after the 1780s. That is the subject of this new book, a sequel and companion piece to Gibson's earlier *Feeding the Russian Fur Trade* (1969). Together with his numerous articles, they establish him as the most prolific historical geographer concerned with the Russian empire of Siberia and the New World. Here he has concentrated exclusively on the struggle of the Russian American Company (from its founding in 1799 until the sale of Alaska in 1867) and the Russian government to supply the New World fur-hunting settlements with the necessities of life and a very few niceties. He has not tried to investigate the *raison d'être* of the colony, the trapping and trading of sea otter and fur seal, or the bureaucracy which directed it and the natives who performed much of the labor.

Russian America from the beginning could not provision itself because of its northerly climate, poor soil, insufficient labor, and poor agricultural technology. From the famed Aleksandr Baranov on, the Company's officials turned fitfully to shipment from Siberian or European Russia (which was egregiously expensive), settlement further south (the Sandwich Islands between 1815 and 1817, and northern California), and trade with foreigners (Bostonians, Hudson Bay merchants, Spaniards in lower California, and Hawaiians). None of these expedients worked satisfactorily and steadily. With national attention directed elsewhere, the fur market in decline, and supply inadequate, Russia wisely (for that day) disposed of her most distant colony.

This book helps chronicle a fascinating episode of empire by focusing on a commonly neglected aspect of the subject. The style is somewhat prosaic, but clear. The maps are adequate and the illustrations charming.

CLIFFORD M. FOUST
*University of Maryland, College Park*

ROBERT V. REMINI. *The Revolutionary Age of Andrew Jackson*. New York: Harper and Row. 1976. Pp. x, 205. $6.95.

*The Revolutionary Age of Andrew Jackson* does more than retell a familiar story. Robert V. Remini has synthesized his prior work in an attempt to demonstrate its "relevance" to the general reader, particularly the high school student for whom history is, all too often, boring stuff. Jackson, he reminds this challenging audience, was a "product and shaper" of a "profound revolution" which gave birth to the dynamic, urban, industrial society of "modern America." He personified the democratic urge to destroy economic privilege and to create equality of opportunity—at least for those who were white and male.

After outlining the social, psychological, and economic dimensions of this revolution, Remini focuses on the struggle of Jackson and his "exciting bunch" to bring politics into harmony with changing social values. Van Buren, Clay, Webster, and Calhoun come alive as prototypical modern politicians, whose superb organizing skills and electioneering techniques revitalized the two-party system and engineered modern popular government. Jackson, "the first modern president," revolutionized American government by asserting presidential authority during the Bank War. Minorities were affected by this revolution but were denied their own. So, in the Nullification crisis Jackson subordinated the moral problem of slavery to the political problem of union, and it fell to this spokesman for political democracy to "solve" the Indian problem through the removal policy. There is a poignant illustration of the "ghastly" unevenness of democratic justice in the

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betrayal and execution of Tsali, the Cherokee hero.

There are problems with this approach. Remini's admiration for Jackson inhibits his pursuit of "relevance." The perceptive student might see portents of the imperial presidency and the Nixon scandals in Jackson's radical transformation of the executive. "Not so!" says Remini. Jackson represented "something else"—the strong presidential leadership necessary to national progress and social betterment. "Notable statesmen" like Lincoln, Wilson, and the two Roosevelts strengthened this worthy concept. Not until World War II did the "unlawful and loathsome" notion of limitless executive power creep in and pervert the Jackson ideal. Jackson's heart may have been in the right place, but there is more of relevance in his presidency than a desire to bring the government and the people closer together. The student might also observe that a historian could discover "modern America" at almost any point in the past by carefully selecting modern characteristics—as William L. O'Neill's recent *The Progressive Years* demonstrates. Remini recognizes this problem and occasionally cautions that these years were only a beginning. The prevailing tone, especially in the introductory chapter, suggests more. Finally, identification of those he quotes to such good effect would have been helpful.

Such criticism borders on niggling about this virtuoso performance. Remini is a master storyteller. This is, in the best sense, a popular work, though the mark of the professional is on every page. He admits that much of what he says or leaves unsaid will cause "howls of disagreement" among historians, but he restricts the parade of scholarship to an instructive bibliography for those students interested in the latest research or in controversial points. The rest is a bold, spirited, and dramatic narrative that will surely capture the attention of students expecting the cautious, lifeless, and unimaginative. Remini is a model of the professional historian as teacher, and he deserves emulation. His book, like the age it recreates, begins and ends "with a bang."

FRANCIS N. STITES
*San Diego State University*

MARY LEE SPENCE and DONALD JACKSON, editors. *The Expeditions of John Charles Frémont.* Volume 2, *The Bear Flag Revolt and the Court-Martial;* volume 2 supplement, *Proceedings of the Court-Martial.* Urbana: University of Illinois Press, 1973. Pp. iii, 519; xvi, 464. $17.50; $12.50.

John C. Frémont's career came to a climax with his 1843–44 expedition through South Pass to the Columbia, along the eastern base of the Cascades and the Sierra Nevada, across to Sutter's Fort, and home by way of the Mojave Desert and the southern Rockies. Unreported for many months, the expedition was spoken of as "lost," which heightened the reception. Ably assisted by his talented wife, Frémont wrote a glowing account of this grand adventure, immediately a most widely read book.

Frémont returned to California in 1845–46 over the California variant of the Oregon Trail. Soon at odds with the Mexican officials, he went to Klamath Lake. Overtaken by a messenger from President Polk, he turned around and was on hand for the Bear Flag Revolt and the so-called conquest of California. Here was a subject that called for a sequel that might have surpassed his earlier *Report*, though in it Frémont would not always have been the dominant figure.

The first half of volume 2 of the *Frémont Papers* carries the narrative to the Cahuenga Capitulation in January 1847, and the establishment of American possession. On the trip west chief reliance had to be on the *Memoirs*, written and colored many years later. On the California experiences a hundred letters are scant coverage; the corresponding segment of the *Larkin Papers*, for instance, runs to two and a half volumes.

The other half of volume 2 and the supplementary volume, a facsimile of a volume in the government document series, are engrossed with the quarrel with and between Commodore Robert Stockton and Lt. General Stephen Watts Kearny, Frémont's return under orders to Fort Leavenworth, his arrest there, and the court-martial in Washington. Here the documentation is much richer. There is illumination of much that was going on in California in 1847, the vagaries of military government and interservice rivalry, and the constraints of military justice.

Biographers and historians have tilled this 1845–48 subject matter industriously, if not exhaustively, and in the process have combed most of the documents here assembled. They have shown at least a ten to one preference for the exploration and description and the infiltration and takeover as against the quarrel and the court-martial. Empirically that ratio seems warranted. Editors of personal papers, on the other hand, have to go with the magnitudes, here one to three in the other direction. The editor is well justified in chiding historians who seem not to have used the proceedings of the court-martial.

The preparation of these two volumes has been beautifully done. With economy of words editor Mary Lee Spence identifies persons, places, and events referenced and disposes of certain ambiguities. Her introduction provides the setting and gives a neat synopsis of Frémont's experiences.

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Her overall judgment is incontrovertible that "in the California episode Frémont was as often right as wrong" (p. xx). Nevertheless, though the Republicans nominated him for President in 1856, such a record was an appalling comedown from his public image as the perfect leader in the opening of the West.

JOHN CAUGHEY
*University of California, Los Angeles*

PHILIP S. FONER. *History of Black Americans: From Africa to the Emergence of the Cotton Kingdom.* (Contributions in American History, number 40.) Westport, Conn.: Greenwood Press. 1975. Pp. 680. $25.00.

C. DUNCAN RICE. *The Rise and Fall of Black Slavery.* New York: Harper and Row. 1975. Pp. xiii, 427. $15.00.

Prefaces can be useful steeds whereby authors may head off reviewers at the pass. When one returns to the preface after reading C. Duncan Rice's sweeping history of New World slavery and its abolition, one reads almost exactly the review one wishes to write. The preface disarmingly asserts that the book arose out of a course of lectures, is weighted perhaps too heavily on anglophonic America, and "is intended for the general reader."

Happily, the reviewer is left with a few observations not covered in the preface. *The Rise and Fall of Black Slavery* is a lucid survey of a vast topic, written with wit (a quality few historians of this subject can muster) and too many digressions (including a page-long quotation from Ralph Waldo Emerson). The author stresses the "ghastly inevitability" of the emergence of slavery in the New World and finds its eventual abolition more in need of explanation than its origin. Indeed, he seems to feel on firmer and more attractive ground when discussing the movements against slavery than when dealing with slavery as an institution. For a survey of this scope the book is more than usually accurate, with the inexplicable and horrifying exception of placing the great emancipation debate in the Virginia legislature immediately *prior* to the Nat Turner rebellion.

In dealing with all the slaveholding powers in the New World, Rice tends a trifle toward overhomogenizing the experiences of various colonies. He stresses the continuity of New World slavery with long-established traditions of slavery in the Old World. This emphasis fits very well with Spain and Portugal, but not very well with England. Indeed the self-conscious pride in English liberties, so characteristic of Elizabethans, had a good deal to do with the character of English slavery in America—and its abolition as well.

While Rice's book focuses on slavery as an institution, Philip S. Foner's deals more directly with the experience of black Americans in the United States. It is the first of a projected four-volume history of Afro-Americans, and is six hundred pages long. It carries the story to about 1820. It tells the story in, as they say, some detail.

Like virtually every other survey of Afro-American history, Foner's story starts out rather badly because it begins in Africa. Reacting to assertions about African barbarism in somewhat the same way that the abolitionist Anthony Benezet did two hundred years ago, Foner insists in talking about great kingdoms and other civilized "achievements," thereby presuming that the more Africans resembled Europeans the better it would be for all of us. There is even an entire section on the great accomplishments of the ancient Egyptians, which were as pertinent to the history of Afro-Americans as the glories of medieval Poland were to the English settlers in America.

The story gets much better in the New World. There are the customary chapters on the West Indies and on Latin America. The establishment and character of slavery in the various English colonies, the rise of antislavery, blacks in the American Revolution, emancipation in the North, slave resistance, slavery and the Constitution, blacks in the War of 1812, Phyllis Wheatley, Benjamin Banneker, Paul Cuffe, Gabriel Prosser, and so on—the entire story is told lucidly and panoramically.

There is a moralistic tone to the book which some readers will like and others not. Slavery is portrayed as hideously oppressive, yet slaves heroically rebellious. Once again one is led to ask whether such a brutal system had no deleterious effects on its victims. By the time the reader gets to the inevitable discussion of Thomas Jefferson and Sally Hemings one knows for certain what the author's conclusion will be: Yes, the author tells us, of course TJ was the father.

This tone apart, Foner's comprehensive account provides thoughtful and balanced judgments, though few readers will agree with all of them. The chapters on the influence of St. Domingo on the United States afford a fine discussion of greatly neglected events in American history. For a book so chock full of factual material, there are remarkably few errors of fact. The entire enterprise rests on remarkably energetic research in primary and particularly secondary sources, and there is a long and useful bibliography.

WINTHROP D. JORDAN
*University of California, Berkeley*

LESLIE HOWARD OWENS. *This Species of Property: Slave Life and Culture in the Old South.* New York: Oxford University Press. 1976. Pp. 291. $12.95.

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Slavery—that historiographical battleground on which scholars make such extravagant claims and where computers have been so misleading (or useless)—is approached dispassionately by Leslie Howard Owens. Beginning with a brief overview of the color basis of slavery, Owens brings new insight to the discussion of the impact of disease and diet on slave behavior. Contrary to Fogel and Engerman, Owens found slave diet generally deficient in protein, calories, and essential vitamins. These deficiencies and inadequate medical care reaped a heavy toll in the quarters and account for much of the truth in the stereotype of the lazy slave. While breaking new ground in these areas, Owens' chapters on the house servant and the slave driver seem less exciting and suggestive than Genovese's treatment of the same subjects. Even so, the author has explored new sources on these and most other topics in the book.

Finding props for self-esteem in religion, music, and folklore, the slave had an extended family, a vibrant culture, and a personality which survived the rigors of bondage. The socialization process in the family, the unity of blacks in the quarters, and the limits the bondsmen placed on the "absolute" authority of the master are well developed by Owens. A staggering array of manuscript sources lends an air of credibility frequently missing in some recent studies of the peculiar institution.

All too often Owens ignores or pleads the lack of sources when confronting such major historiographical questions as African survivals, the extent of resistance (many rebellions were "absorbed" by the system), or family separations. Frequently the result is a disturbingly bland veneer to an inherently exciting discussion. Nevertheless, the absence of polemics, the additional information on traditional topics, and the impressive list of sources will insure *This Species of Property* a place beside the recent studies of Peter Wood, Julia Smith, and Eugene Genovese.

JOHN W. BLASSINGAME
*Yale University*

LEONARD I. SWEET. *Black Images of America, 1784–1870.* (The Norton Essays in American History.) New York: W. W. Norton and Company. 1976. Pp. x, 214. Cloth $8.95, paper $2.95.

Leonard Sweet's superb book casts the nineteenth-century black's perception of America against the backdrop of George Bancroft's epic vision. Bancroft's providential view of national history saw America as a stroke of divine will, its destiny being God's signature. Black spokesmen, no less imbued than whites with this vision, would emphasize the onus rather than the blessings of the ordination. "Manifest destiny" was not to fill the continent, but to realize national ideals of human rights and social justice. Thus, the Afro-American's place in national history was crucial, being the single best evidence of the nation's unmitigated failure, and pointing the sure path to its redemption. Black leadership played the role of a Jeremiah, reminding whites of the awful responsibility in the American experiment and reassuring themselves of their rightful place.

Some were persuaded nevertheless, that the special call of black Americans was the "work of civilizing and evangelizing Africa." They joined in schemes to preach to or colonize Africa and, despairing of black men's future in America, engaged in debate with those blacks who viewed themselves as necessary instruments of American reform.

Leonard Sweet's analysis is fresh and compelling. He challenges facile dualisms: nationalism/accommodation, separatism/integration. He discovers a subtle nationalism in those who would remain in America and insist on their rightful place. They shared with many Walt Whitman's view of America as "a nation of nations." Sweet thus broadens the concept of black nationalism in useful ways.

Looking ahead into the 1920s, Sweet sees a dramatic shift, de-emphasizing common destiny and stressing alienation. Marcus Garvey's nationalism tolerated no dual loyalties. Only a present-centered myopia would find such a view in the nineteenth century.

Sophisticated, provocative, and original, this book is essential reading. It is written in a lucid and careful prose and contains a helpful essay on bibliography.

NATHAN IRVIN HUGGINS
*Columbia University*

RICHARD H. SEWELL. *Ballots for Freedom: Antislavery Politics in the United States, 1837-1860.* New York: Oxford University Press. 1976. Pp. xvi, 379. $14.95.

This is a notable book for several reasons. First, it argues persuasively for the continuity of political abolitionism from the pre-Garrisonian era through the election of Abraham Lincoln; second, it presents the strongest case possible for the primacy of the moral impulse in antislavery politics of the 1840s and 1850s; third, and perhaps most remarkable, both achievements are accomplished through the use of thoroughly traditional materials and approaches.

For those readers who enjoy detecting resemblance in scholarly productions, much as some delight in identifying look-alikes at family reunions, this book's theses will summon recollections of the writings of Gilbert H. Barnes and especially of Dwight L. Dumond. These two historians' works, however otherwise dissimilar from Richard Sewell's, reached conclusions somewhat similar to

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his. Dumond in particular, in his monumental but occasionally misunderstood and often slighted *Antislavery: The Crusade for Freedom in America,* stressed both the political ramifications of slavery itself and the political activities of pre-Garrisonian abolitionists. Sewell quite fittingly passes lightly over the years before 1830, a period Dumond emphasized, to focus first on the still-neglected political concerns of Modern Abolitionism (as contemporaries referred to the era of Garrison and the American Anti-Slavery Society), and then on the antebellum decades, when antislavery emerged to be seen by even the dullest eye as a major force in electoral politics. The emphasis throughout is on continuity in personnel, goals, and means.

For all their outspoken hostility to slavery and Southern power the Free Soil and Republican parties have commonly been viewed as embodying only a remnant of that vital moral impulse which was so clearly manifest in the American Anti-Slavery Society and (to surrender to Sewell's argument) in its successor, the Liberty party. To support their argument that a watering-down process had occurred, critics observe that later antislavery politicians sought only to prevent the spread of slavery, not to destroy it in the Southern states. They further bolster their contention by combing the public and private utterances of Free Soilers and Republicans for expressions that evidence racist attitudes. They have little trouble finding them.

Sewell argues against the justice of these criticisms. He demonstrates that Free Soilers and Republicans were no more willing than their abolitionist predecessors to tolerate indefinitely the existence of slavery. He shows that many politicians who called for the containment of slavery believed that by so doing they would destroy it. The economic interests of antislavery politicians, he insists, never obscured their moral indignation over slavery, their abolitionist intent, or their commitment to basic civil rights for blacks. The anti-Negro sentiments of some antislavery politicians are not easily disposed of, but Sewell makes an able effort to do so. Probably no writer will ever present a stronger case for the political abolitionists' philanthropy than appears here. It is no criticism of the author's skill or industry to predict that his argument will not convince everyone, for in this instance it is ultimately impossible to explain away the need for explanation.

The rich and detailed narrative through which the above arguments emerge is supported by massive documentation supplied by research in some ninety-eight manuscript collections—nearly all of them the correspondence of "leaders"—and dozens of newspaper files. The book makes no concessions whatever to the method or style of the new political history. Though it is not at all likely that a different approach would have yielded a wiser, more useful, or more valid product, those historians who adhere to the traditional in research and presentation, even when they produce books of great worth and integrity as Sewell has done, must ready themselves to withstand onslaughts from the scornful opposition.

MERTON L. DILLON
*Ohio State University*

DAVID M. POTTER. *The Impending Crisis, 1848–1861.* Completed and edited by DON E. FEHRENBACHER. (New American Nation Series.) New York: Harper and Row. 1976. Pp. xv, 638. $15.00.

What a blessing that David M. Potter, one of the most admirable historians writing in the twentieth century, lived long enough to see this manuscript far along its way. How fortunate, too, that the astute Don E. Fehrenbacher completed the draft and edited the whole. Both scholars were—and Fehrenbacher is—widely recognized as authoritative in the period. If additional evidence were needed, *The Impending Crisis* would supply it.

In an introduction to this New American Nation Series book, editors Henry Steele Commager and Richard B. Morris describe the 1850s as the "most controversial chapter of our history." While some scholars may balk at "most," it is doubtful if anyone can convincingly deny that the 1848–61 span was, and is, extremely controversial. Allan Nevins, Roy F. Nichols, Avery Craven, and others have been over nearly all of the same ground. Wherein, then, lie the Potter contributions?

Major assets are at least three in number: 1.) Virtually every important political development is analyzed with consummate care; 2.) unusually valuable historiographical appraisals appear in footnotes throughout the volume; 3.) splendid synthesis—the best ever offered for the period—distinguishes threescore or more of the textual pages.

Liabilities? In any work of such scope, specialists may be tempted to pick a nit. The development of Young America, for example, seems less than adequate. The nonappearance of William Wilson Corcoran in the book is even more surprising than the omission from the bibliography of Henry Cohen's fine Corcoran study.

Still, no exclusion can compare in significance with the masterfully executed inclusions—the integration of 1853–56 foreign and domestic issues (p. 177 ff.), for instance, and the clear perspective (p. 436) on 1860s immigrant vote. Numerous observations respecting railroads, Kansas, the Dred Scott decision, and four presidential elections are memorably apt. The cause or causes of the Civil War? Persons seeking a simplistic answer will be disappointed. Yet it is doubtful that many readers will

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lay down the book without understanding that, in Potter's judgment, slavery had at least as much to do with the total crisis as any other single factor.

What I like most is Potter's achievement in reproducing actions and thinking of the people of the era without altering the focus from presentist angles. Even more than the felicitous style, his distinctively faithful re-creation of an era and his mature assessment of it make *The Impending Crisis* of Pulitzer and Bancroft Prize quality.

HOLMAN HAMILTON
*University of Kentucky*

NORMAN B. FERRIS. *Desperate Diplomacy: William H. Seward's Foreign Policy, 1861*. Knoxville: University of Tennessee Press. 1976. Pp. ix, 265. $12.95.

In neither methodology nor interpretation is this detailed account of the first year of Union diplomacy the new departure it purports to be. It is, instead, a conventional treatment of a conventional view whose only novelty is the extremity to which it carries the notion that British diplomats and policymakers were actuated by an unreasoning malevolence toward the United States. At the other extreme, Seward is portrayed as a mild-mannered, guileless statesman who by dint of superhuman forbearance and brilliantly dextrous diplomacy singlehandedly (Lincoln was only another of his burdens) saved the Union from dismemberment by wily Europeans.

Unfortunately, Norman B. Ferris' evidence either does not support this Manichean view, or is open to other, equally plausible interpretations. Thus, Ferris' animus notwithstanding, the reader is apt to come away with admiration for British moderation and good sense despite Seward's many provocations. In his lawyer's brief, Ferris uncritically accepts Seward's view of himself, and hence cannot admit that Seward (here as elsewhere) sought to manipulate public opinion to further his diplomatic objectives. Furthermore, his foredoomed effort to depict Seward as a pacific man deprives Ferris of opportunities for fresh insights by compelling him virtually to ignore the two most controversial episodes of 1861: Seward's "Thoughts" on April 1 and the Trent Affair. Instead, trivial events like the Bunch Affair get whole chapters that could easily have been condensed to footnotes.

Ferris' obsession with British animosity results in another, equally damaging imbalance. He devotes much of his book to cataloging Lord Lyons' dislike for Seward, while ignoring the complexity of British policymaking and the domestic constraints upon it. Ferris' knowledge of the literature displays some surprising gaps. For example, he accepts Louis Schmidt's 1918 thesis on the influence of Northern wheat on British policy, seemingly unaware of Eli Ginzberg's 1936 refutation of it.

When the full story of Civil War diplomacy is finally written, it will be placed within the context of Seward's global ideas and long-range policies as well as American domestic developments, while doing the same for British and European diplomacy. Only then will true understanding be possible. That task still awaits its historian.

ERNEST N. PAOLINO
*New Brunswick, N.J.*

ROBERT W. DUBAY. *John Jones Pettus, Mississippi Fire-Eater: His Life and Times, 1813–1867*. Jackson: University Press of Mississippi. 1975. Pp. xii, 234. $12.50.

John Jones Pettus, first of the Civil War governors of Mississippi, has not been one of the better known protagonists of the national tragedy of the 1860s. This small volume is unlikely to add appreciably to his recognition factor.

Despite the "life and times" promise of its title, Robert W. Dubay's study is hardly more than a thin chronology of the surface experience of Mississippi during the war years, lacking the substance and interpretive sophistication of older works by Percy Rainwater and John K. Bettersworth. Even in the area in which it should be strongest, the particular and intimate relationship of his central figure to the overall story, Dubay's work adds little if anything to our understanding of Pettus as Southerner, politician, executive, or human being. His presence, indeed, is hardly felt in these pages, and he remains disembodied, distant, and consequently supremely uninteresting.

This is all the more regrettable because of the potential which Pettus would seem to offer to the historian. Born in Tennessee, he migrated to Mississippi in the mid-1830s, entered politics in the 1840s, and became generally identified with strong support of internal improvements and railroads as well as consistent radical opposition to any compromise of the demands of Southern nationalism. Steady and plodding loyalty to his political faction brought him to the governorship in 1859 and to inescapable confrontation with the crisis of 1860–61. Through it all he remained unwavering in his commitment to the feasibility of Southern independence, jealous of his state's prerogatives in the new Confederacy, and pedestrian if not worse in the quality of his leadership.

A plain, rough-hewn product of the frontier, Pettus was never the spokesman for a united political community of homogeneous ruling class. His experiences in relating to the persistent Whiggery of his state, to closet Unionists such as J. F. H.

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Claiborne, to the distant authority of Richmond, and to the closer egos of military commanders like John Pemberton—all of this invites hard and searching analysis and clarifying explication. Unhappily, they are not to be found in this disappointing volume.

JOSEPH G. TREGLE, JR.
*University of New Orleans*

HERMAN HATTAWAY. *General Stephen D. Lee*. Jackson: University Press of Mississippi. 1976. Pp. xi, 283. $12.50.

Stephen D. Lee (of the South Carolina, not the Virginia, family) served the cause of the Confederacy superbly. A West Point graduate, Lee resigned his federal commission in time to be General Beauregard's personal emissary to accept Major Richard H. Anderson's surrender of Fort Sumter. Subsequently, Lee performed laudably as an artillery officer in the Seven Days campaign and at Second Manassas and Antietam. When he was transferred to the West, Lee's tenacious defense of Vicksburg won the admiration both of southerners and of the city's eventual captors. During the remainder of the conflict, Lee served the western army as cavalry officer and as department and corps commander, performing so creditably that at age thirty he was made lieutenant general—the youngest officer on either side to attain that rank.

Herman Hattaway's chronicle of Lee's military career is a welcome addition to Civil War military history. Indeed, the author makes a convincing case (in spite of occasional ill-chosen adulatory and unobjective quotations characterizing his subject) that General Lee's contributions to the Confederate effort have long deserved this kind of sustained attention.

Hattaway's chapter on the United Confederate Veteran's history committee is a gem; Lee was chairman of the panel which acted as a watchdog against unacceptable school textbooks with northern biases. Equally interesting is the material on the general's nineteen-year tenure as president of Mississippi A&M College. One wishes, however, that the author had explored his subject's early life and the influences that molded his exceptional character. Also deserving of more attention are the bases of Lee's support in his forays into state politics, in particular his relationships with the Grange and Farmers' Alliance. Blacks are absent from this biography—one can relate to the "invisible man" metaphor in reviewing the career of this Southern leader.

Hattaway's chapter on Lee's "strangely flavored" Bourbonism is fuzzy around the edges. The author might have heeded—as we all should—his warning against a universal label to characterize New South leaders. Too much ink, the reviewer's included, has been expended as the author of each new biography explains at length why *his* Bourbon is not like other Bourbons. An additional note on labeling: some readers may be amused by Hattaway's reference to "Pitchfork" Ben Tillman as the "South Carolina statesman."

Although an important Mississippian, Stephen D. Lee remains a figure only of local and regional interest. Lee's lack of any genuine national contribution is sadly typical of most competent southerners of the New South era.

HUGH B. HAMMETT
*Rochester Institute of Technology*

ROBERT S. HOLZMAN. *Adapt or Perish: The Life of General Roger A. Pryor, C.S.A.* Hamden, Conn.: Archon Books. 1976. Pp. 209. $13.50.

Born in 1828 to an old Virginia family, Roger Pryor had a fascinating life. He received his law degree from the University of Virginia at nineteen, but with his oratorical ability temporarily impaired by a throat ailment turned to journalism. In the 1850s he held editorial positions on several papers, including Thomas Ritchie's Richmond *Enquirer* and the Washington *Union*. Elected to a vacant congressional seat in 1859, Pryor became a tempestuous defender of southern rights in the strife-torn House and hastened to Charleston to aid the Confederate cause even before Virginia's secession. Thereafter, he served as a colonel and brigadier general in the Virginia campaigns of 1861–62 only to resign his commission the following year in dismay at the depletion of his command by transfers. He re-enlisted as a private in the cavalry.

Unable to rebuild his career in postwar Richmond, Pryor, now an advocate of sectional reconciliation, moved to New York City late in 1865 to accept employment with the original *Daily News*. After a few months he began a law practice that he built into one of the city's most distinguished. Pryor argued several cases that drew national attention, including the plaintiff's cause in the adultery trial of the Reverend Henry Ward Beecher, and in 1887 represented the Haymarket defendants in their unsuccessful appeal to the Supreme Court. Later a justice on various state courts, he died in retirement in New York City in 1919.

Despite having a subject whose career warrants comparison in eventfulness with that of Ben Butler, Pryor's frequent legal associate, Robert Holzman, a retired professor of taxation, turns in a disappointing study. It is marred by inadequate research (no effort to compensate for the lack of Pryor manuscripts is evident), an excessive use of quotations, and an almost total absence of analy-

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sis. For instance, the author merely notes that Pryor practiced law in New York in the company of the era's most eminent attorneys—Joseph Choate, David Dudley Field and Elihu Root—without assessing Pryor's contributions to the bar and to legal thought during this significant period in the history of the legal profession.

In sum, this is a shallow book that will have only limited value for historians. Civil War buffs will likewise find it unrewarding. It does perform a useful service by collecting the scattered facts of Pryor's career and will—it is hoped—encourage some enterprising historian to undertake the type of study Pryor deserves.

LLOYD J. GRAYBAR
*Eastern Kentucky University*

HANS L. TREFOUSSE. *Impeachment of a President: Andrew Johnson, the Blacks, and Reconstruction.* Knoxville: University of Tennessee Press. 1975. Pp. xii, 252. $10.95.

A foremost interpreter of the Republican role in Reconstruction, Hans Trefousse, has written a unique volume on the effect of impeachment on Radical Reconstruction. In this extended essay, largely based on secondary sources, Trefousse contends that the attempt to remove President Johnson from office gave the Tennessean the opportunity to counterattack by vetoing radical legislation. More important, the failure to remove the president from office prevented the Radicals from using the federal government to effect their reforms.

Trefousse dispenses with the question of whether the president had indeed committed an impeachable offense and ought to have been removed from office. Instead he asserts that the Radicals had little choice but to impeach the president, since they believed that Johnson was bent on restoring racial controls in the South. He concludes that moderate Republicans had to join the Radicals, because the moderates knew that Johnson would rebuild the Democratic party and shift the balance of power too heavily in favor of the executive. Thus, the Republicans were near unity in their initial bid to unseat the president.

But the Republicans badly misjudged the political abilities of Johnson and the mood of the people of the North. Trefousse says that Johnson realized that he would not be removed from office, so he forced the Radicals into reckless actions which lost them popular support. Johnson also understood that many Republicans hated and feared Benjamin Wade, the man who would become president in the event of a conviction; for this reason, the Republicans equivocated on the final decision to remove Johnson. That knowledge freed Johnson to take the offensive against his opponents, buy the time necessary for conservatism to be restored in the South, and link political radicalism with the cause of reform for the former slave.

There are a few weak spots in this well-argued thesis. Trefousse commits an error in creating a monolithic South after the war, and he completely ignores the degree to which the Southern people had lost faith in their political leaders. The author does not study Johnson's ambivalence about the fact that his activities would restore his enemies to power in the South. The Republicans, like Trefousse, misunderstood the South. But they did understand how the impeachment movement would affect popular opinion on presidential powers. Through congressional government the Republicans were able to keep their party in power for nearly half a century, even if they could no longer use public opinion to perpetuate reform politics. Because the Radicals remained divided in purpose, Trefousse is incorrect when he concludes that failure to convict meant the loss of radicalism. The real loser was the freedman.

JON L. WAKELYN
*Catholic University of America*

ROGER L. HART. *Redeemers, Bourbons & Populists: Tennessee, 1870–1896.* Baton Rouge: Louisiana State University Press. 1975. Pp. xvi, 290. $12.95.

This study of Tennessee politics from the end of Reconstruction to 1896 seeks to delineate political patterns and determine how far the rivalries reflected fundamental economic or social tensions. The Redeemers, having wrested the state government from the Republicans, failed to prevent disruption of the anti-Republicans over the issue of payment of the state debt. Conservative Democrats established control of a momentarily reunited state party in the 1880s, but they could not prevent the Farmers' Alliance from coming to power within the party. The Alliance movement was effectively crushed, however, when some of its leaders put the Alliance ahead of the party. The ensuing third-party effort, the People's party, failed utterly in 1892 to woo Democrats from their party loyalty. The 1896 national campaign of William Jennings Bryan as the presidential nominee of the Democratic party ended an era of almost kaleidoscopic factionalism within the Tennessee Democratic party.

The author has effectively used a substantial body of literary sources and provided numerous analyses of legislative voting behavior and aggregate voting data. The existing relevant interpretive viewpoints are lucidly presented. The entire narrative is gracefully rendered, and the conclusions are

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offered without equivocation. The conclusions are that Tennessee politics of the period were complicated and ever changing, that no persistent factions of individual leaders were behind the surface political storms, and that economic or social class conflicts cannot be the pervasive explanation of political rivalries. The author is convinced, given the undermining of evidence for other interpretations, that the persistence of antebellum party loyalties, the durable Unionist-Secessionist antagonisms of the war era, and friends-and-neighbors localism contributed greatly to a voter's self-identification. "The major basis of political attitudes . . . was the need to arrange experience in coherent, meaningful ways—ways that supported the individual's self-esteem and social status," hence "the desire to put into public office men from a group with which one identified, in order to bestow recognition on that group and enhance its status."

THOMAS B. ALEXANDER
*University of Missouri, Columbia*

STEPHEN J. DECANIO. *Agriculture in the Postbellum South: The Economics of Production and Supply*. (MIT Monographs in Economics.) Cambridge: MIT Press. 1975. Pp. xii, 335. $15.00.

This monograph, an outgrowth of a doctoral dissertation, is designed to shed new light on the social and economic upheavals in the South after the Civil War through the techniques of quantification, which the author believes are more useful than the traditional historical approach. He sets out by summarizing in rather cursory fashion what he views as the major economic issues that faced the South during these troubled years and then confines himself to answering such questions as whether Southern agricultural labor was exploited, whether white farm labor was more productive than black, whether cotton was more productive and profitable than alternative crops, and whether rural furnishing merchants exercised monopoly power on the market. At the end of a chapter of more than 100 rambling pages and 314 footnotes, which easily could have been reduced by half, he concludes that the "narrative, anecdotal, nonquantitative and impressionistic" approach of the historians fails to tell us which of the alternative hypotheses were true in the aggregate, whether the exploitation of labor was general or exceptional, or whether white or black farm labor was more productive. He then informs us that he has developed and applied more powerful techniques in helping to understand these historical problems.

Whether he has shed new light by these "powerful techniques" is a matter of opinion, depending on one's value scale. This study will be well received by those who believe that history should consist primarily of those phases that are quantifiable, despite the fact that things that do not lend themselves to measurement often are more important than those that do; those who believe that such techniques distort history rather than set the record straight will view this monograph with suspicion. The author concedes that many problems cannot be answered strictly by economic explanations, yet he proceeds to try to answer them as though they could be. What follows, in the opinon of the reviewer, is a tedious if not baffling array of charts, tables, and mathematical formulas designed primarily for the technicians, which will leave the general reader cold. Are the statistical data for those years complete and reliable? Are those built-in biases, frustrations, and psychological consequences that followed in the wake of defeat and the politics at the county, state, and federal levels so unimportant to the economic sector of the South as to be avoided completely? Perhaps the author is right in saying that the traditional historical methods have not provided the answers to the questions he is seeking answers to, but I do not believe that his "powerful techniques" have provided those answers either.

THEODORE SALOUTOS
*University California, Los Angeles*

JAMES J. BARNES. *Authors, Publishers and Politicians: The Quest for an Anglo-American Copyright Agreement, 1815–1854*. Columbus: Ohio State University Press. Pp. xv, 311. $13.00.

*Authors, Publishers and Politicians* is the story of a quest that failed—the attempts to secure an Anglo-American copyright agreement between 1815 and 1854. It begins when the matter first received serious attention, after the conclusion of "the Second War for Independence," and ends with the failure of the U.S. Senate to ratify the copyright treaty of 1854. James J. Barnes traces, in admirable detail, the activities in both countries regarding the issue. We learn that James Fenimore Cooper, "one of the few . . . to make an impression on the British reading public," was the first American author seriously interested in securing an Anglo-American agreement. Next, the author discusses the details involved in passing the 1831 law protecting American authors in their own country. It was steered through Congress by Connecticut Representative William Ellsworth, the son-in-law of Noah Webster, who sought copyright protection for his "spelling books." The author also covers the maneuvers of literary pirates on both sides of

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the Atlantic and the machinations and lobbying by bookmen and newspaper publishers in search of cheap filler matter. Indeed, there is little one could want to know about the status of the copyright controversy that is not covered in great detail.

The author is to be congratulated for reducing forty years of complex, at times tortured, lobbying and litigation into clear, readable prose. He summarizes the results in these words: "During the first half of the nineteenth century, British authors had little prospect of protecting their publications abroad, especially in America. However, depending upon the state of law as interpreted by the British courts, foreign authors including Americans could secure a valid copyright in Britain."

JUSTIN E. WALSH
*Indianapolis, Indiana*

SALLY GREGORY KOHLSTEDT. *The Formation of the American Scientific Community: The American Association for the Advancement of Science, 1848–60.* Urbana: University of Illinois Press. 1976. Pp. xiii, 264. $10.95.

In the mid-1840s, challenged by the European example, Americans began in several ways to lay the institutional foundation of modern American science. Sally Gregory Kohlstedt's study of a cornerstone in that foundation has as its leitmotif the dilemma of scientists in egalitarian and republican America, who had to cultivate the general public for support while struggling to confirm and refine their new professionalism. The popular self-culture movement of the 1830s had tended both to nourish and to debase American science. Scientists' heightened fear of amateurism delayed the formal organization of the AAAS until 1848 by requiring a prolonged flank approach through a national association of professional geologists. Even so, the new organization had to go beyond its British model in concessions to democracy or anti-elitism. During the annual AAAS meetings, the "savants" were flattered by public interest, yet reluctant to address questions of popular concern, such as evolution and racism. After the first three years of organization and good fellowship came a long contest between an able but arrogant and self-perpetuating inner circle and those who feared that it might stifle fledgling professionals along with the amateurs. By 1860 the strain had told on the AAAS, and the AAAS revival after wartime suspension did not fully restore its centrality to American science.

Kohlstedt's account of all this is heavy going. Solecisms mottle its leaden prose. But reading it is worth the effort. Kohlstedt has thoroughly mined the immense body of manuscript sources left by scores of record-conscious antebellum scientists. Her other sources are impressively numerous and varied. She draws on sociological, economic, philosophical, and psychological studies for background. She seizes on the possibilities opened by a well-defined and well-recorded community for an elaborate statistical analysis. Above all, she makes explicit the manifold implications of her data. In substance this study is definitive on a historical subject of first importance.

ROBERT V. BRUCE
*Boston University*

CEDRIC CUMMINS. *The University of South Dakota, 1862–1966.* Vermillion: University of South Dakota Press. 1975. Pp. v, 334. Cloth $8.95, paper $4.95.

EMORY LINDQUIST. *Bethany in Kansas: The History of a College.* Lindsborg, Kan.: Bethany College. 1975. Pp. viii, 309.

The institutions discussed in these volumes were founded within a year of one another on the subcollegiate level—the first, as Bethany Academy, in 1881 and the second, as the University of Dakota, in 1882. The latter had been projected in 1862 by the first legislature of sparsely settled Dakota Territory, but the authorization then given was little more than a manifestation of the frontiersman's faith in progress. A different faith—that God would aid their efforts—was exhibited by Pastor Carl Swensson and the small congregation of Bethany Church in Lindsborg, Kansas, when they set out to provide opportunities for Christian education for the children of the Swedish Lutheran immigrants who had settled in that area.

Both ventures proved successful, but only after decades of struggle against financial crises and other adversities. The survival of Bethany and its evolution into a college were made possible by the efforts of dedicated leaders, the loyalty of poorly paid faculty, and contributions from residents of the local community, the Lutheran Church in Kansas, and individual benefactors.

The University of Dakota, which adopted its present name after admission of South Dakota into the Union in 1889, obtained its original campus and buildings through gifts of land and a bond issue approved by citizens of Vermillion and Clay County. After one year's operation, the school was chartered by the territorial legislature and given meager but continuing support from tax revenues. Statehood for South Dakota also made the university eligible for federal aid.

Although each institution had its own serious problems, each also had its successes. Bethany established a reputation in music and the fine arts, and the yearly rendition of the "Messiah" brought

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favorable notice far beyond the borders of Kansas. The University of South Dakota, meanwhile, expanded its services through teacher training, professional colleges, and graduate work on a modest scale. Enough graduates of each school achieved success in study elsewhere and in their professions to indicate that they had received the elements of a good education.

To a large degree the development of these two institutions in the late nineteenth century and beyond falls into the pattern followed by numerous contemporary struggling colleges in the Midwest and South. Later, they both shared in the prosperity and depression following World War I and were swept along by the same currents that brought increased enrollments, better financing, and drastic internal change to all American colleges after the second great war. Both are now respected for the quality of their offerings and the service rendered to their constituents.

These authoritative, well-written accounts contribute significantly to the history of higher education in this country. Their authors, mature historians with long involvement in academic life, have made good use of documentary sources and interviews and have flavored their expositions with first-hand knowledge. Each man appears to have enjoyed his task.

Cedric Cummins states in his introduction that he has attempted "to serve three main functions": to "provide better understanding and nostalgic memories for the institution's many alumni and friends," to produce "a one-volume encyclopedia of reference material," and to provide "a case study of the major trends in higher education played out in the environment" in which his university is located. Unknowingly, he could have been speaking for Emory Lindquist as well as himself, for both have served these functions well.

JAMES F. HOPKINS
*University of Kentucky*

NEIL COUGHLAN. *Young John Dewey: An Essay in American Intellectual History*. Chicago: University of Chicago Press. 1975. Pp. xii, 187. $10.75.

This slender but gracefully written volume offers fresh insights into the emergence of John Dewey's novel contributions to American philosophy. Neil Coughlan's hunch was to get at the evolution of the new philosophy by tracing the personal development of the young scholar who created it. He sees Dewey's personal struggles toward pragmatism shaped by two larger events: the crisis of evangelical Protestantism under the challenge of Darwinism, and the supplanting of church-oriented colleges by scientific-research-centered universities.

Coughlan argues that scholars have overlooked a strong motivation in Dewey—a reach for piety, "not so much in the narrow sense of religous faith as in the broader meaning of faithfulness to an upbringing and culture" (p. 47).

By establishing new connections from standard sources as well as by mining primary sources, Coughlan first delineates the evangelical faith of Dewey's mother with her demanding query, "Are you right with Jesus?" Then in tracing Dewey's moves from Scottish intuitionalism, to Hegelianism, to pragmatism Coughlan sees Dewey struggling to portray human life as marked by purpose and meaning, as did the old faith, even while he stood face to face with the impact of evolutionary science on man's image of himself.

Coughlan is eminently effective when he portrays the personal anguishes of the Christian-secular struggle in the personal histories of Dewey's major mentors: H. A. Torrey, George S. Morris, and G. Stanley Hall, as well as Dewey's close colleague George Herbert Mead. A dramatic consequence for Dewey as a doctoral student at Johns Hopkins was Morris' urgings toward a sophisticated Hegelian apologetics for orthodoxy versus the pull by Hall toward hard-line physiological research in psychology. The painful dilemma for Dewey became whether to remain a Hegelian philosopher in "faithfulness to the old ideal" or to become a laboratory scientist. Characteristically Dewey avoided the either/or choice. He chose primary identity as a philosopher but, from Hall, chose the omnicompetence of science as philosophic method.

Coughlan provides a brilliant analysis of the evolution of Dewey's "reflex arc" concept from his Hegelianism at Michigan to its full-blown development in 1896 when, as "reflex circuit," it became the naturalistic base for purpose and intelligence in pragmatism. Concomitant with this was the emergence of the *social* component in the social psychology and social philosophy of pragmatism. An early manifestation was Dewey's four-year misadventure (1888–1892) with the crank, Franklin Ford, who nearly talked Dewey into copublishing *Thought News*, designed to apply "scientific thought to the world of social fact." This is the best account to date of this strange incident.

The reader will find also an illuminating chapter on Dewey's influence on Mead when Dewey was pioneering the reflex arc concept, and a critical chapter on the strengths and weaknesses of Dewey's ethics.

The book was a joy to this reader. Two gaps were the scant treatment of the critical influence of William James' *Principles of Psychology*, and a neglect of the concept of democracy as a pivotal

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aspect in Dewey's reach for a naturalistic-secular piety.

ARTHUR G. WIRTH
*Washington University*

LEON A. JICK. *The Americanization of the Synagogue, 1820–1870.* Hanover, N.H.: University Press of New England, for Brandeis University Press. 1976. Pp. xi, 247. $12.50.

Leon A. Jick has written a fresh synthesis of the American Jewish experience during the heart of the nineteenth century. It was during this period that the relatively small Sephardic Jewish community (perhaps 2500) was overwhelmed by more than 50,000 German Jewish immigrants. It was also during these years that Reform Judaism took root, that B'nai B'rith, the largest American Jewish fraternal order of the century, began, and that attempts were made to unite the almost anarchic American Jewish community. None of the innovations proceeded without conflict. Older Jewish settlements resented the presence of newer immigrants, most Jews responded to the needs of their local communities and did not seek out national leadership, and Reform Judaism came more in response to the desire for Americanization and respectability than because of any imposition of theological viewpoints carried from abroad.

Jick's major contribution is to expose the myths of unity, solidarity, and orthodoxy, which some other historians have described or implied, and to explain how an unwieldy and diverse number of local congregations eventually came to accept Reform Judaism. With a firm grounding in contemporary American events, the author has been able to describe how immigrant Jews, given opportunities similar to those of other aliens, showed as great a desire to be part of, and accepted by, the dominant community as were others who had an eye on social mobility. The great economic opportunities afforded by the accelerating pace of industrialism in the 1860s and 1870s ultimately provided the vast number of Jewish families a position of "relative affluence" which, in turn, speeded their own desires to worship God in a more "dignified" and "Americanized" manner. The movement culminated with the passage of the Pittsburgh platform in 1885 which rejected all ceremonies "not adapted to the views or habits of modern civilization."

Until recent years most American Jewish historiography consisted of works neither critical nor analytical. Too many books were written to celebrate past glories and impress Christians with the devotion of American Jews to the United States. Jick is certainly not in this school. He has taken a penetrating look at American Jews in the nineteenth century and has ably demonstrated why and how they shed their traditional heritage. His book is an insightful and impressive analysis of a crucial development in American Jewish history.

LEONARD DINNERSTEIN
*University of Arizona*

IRVING HOWE. *World of Our Fathers.* New York: Harcourt Brace Jovanovich. 1976. Pp. xx, 714. $14.95.

In *World of Our Fathers*, Irving Howe tells the story of the mass migration, commencing in the 1880s, of two million Jews from eastern Europe to the United States and of the society and culture which the immigrants created here. He views their departure not merely as a response to poverty and oppression, but as the expression of a collective impulse toward new possibilities of life. The process involved a triple uprooting: from home; from Jewish religious traditions; from their customary work and culture.

Howe sees the immigrant Jewish milieu as an essentially secular community, whose growing integration into American society came at the cost, after a mere thirty or forty years, of internal coherence. "It was a culture that no longer had the inner principle of order—the assumption that God had stamped a unique destiny upon the Jews—which had once kept it in severe control."

Howe is especially attracted to two realms of immigrant activity, literary and political. Of Jewish religious life, he speaks respectfully yet distantly; it is towards "secular Jewishness" that his imaginative sympathy reaches. One of the book's four sections, "The Culture of Yiddish," stresses the unique achievement of a literature created from the midst of an uprooted people and the remnants of a shattered culture, the only modern literature whose esthetes were housepainters and paperhangers.

The book's longest sustained narrative deals with Jewish unionism, Jewish socialism, and Jewish communism. Howe contends that Jewish immigrant culture transformed traditional Jewish messianism into the surrogate faith of political radicalism. Yet running like a red thread through the entire book is the murderous rage of Jewish radicals against Jewish religion, sensibilities, peoplehood. A typical expression of it was the Yiddish communist paper *Freiheit* applauding the 1929 massacre of Jews by Arabs in Hebron. Howe abundantly documents leftist Jew-baiting, yet sees it as an accidental aberration from the Jewish ethos. But one may also take the view that the radicals' substitution of man for God is idolatry

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and therefore necessarily, not accidentally, at war with Judaism.

American Jewish life, unfortunate in many things, has at least been fortunate in having its brief history chronicled by one of the wisest and most humane intellects in American letters. Irving Howe has evaluated the immigrant Jewish milieu with critical detachment tempered by "natural piety"; his attachment to the continuity of Jewish life even as he beholds its deliquescence gives to his book a special beauty.

EDWARD ALEXANDER
*University of Washington*

RUDOLF GLANZ. *The Jewish Woman in America: Two Female Immigrant Generations, 1820–1929*. Volume 1, *The Eastern European Jewish Woman*. New York: Ktav Publishing House and National Council of Jewish Women. 1976. Pp. vii, 209. $15.00.

Although long a subject of sociological and psychological analysis and popular parody, the American Jewish woman has received scant attention from the historian. A beginning has been made, however, in this pioneer effort by Rudolf Glanz.

This monograph is the first in a projected two-volume study of the Jewish woman in the United States and deals with the Eastern European Jewish woman during the years 1880–1929. Focusing principally on New York, Boston, and Chicago, and utilizing a wealth of primary sources—newspapers, journals, memoirs, institutional and governmental records and publications—the author discusses the status, role, and activities of Jewish women in the economy, labor movement, home, and Jewish community. He also offers insights into how the women perceived themselves and were perceived by others, Jews and non-Jews. Most importantly, the work includes forty-five pages of extensive notes and a fifteen-page bibliography of period materials, which are basic for anyone wishing to pursue the subject further.

This in itself makes the book indispensable for those interested in the history of Jewish and immigrant women in America. However, the quality of the narrative is affected by careless editing and proofreading. For example, the method of citation is inconsistent, with some references appearing in footnotes and others in the text. Footnotes are incorrectly numbered. In a number of places the quoted material is not enclosed in quotation marks, set off from the text, or properly cited, making it difficult to determine where the author's words end and those of his source begin. And the material in a number of chapters, notably those concerned with social life, cultural activities and "The Feminine Sphere," could have been integrated and organized more effectually.

Nevertheless, *The Jewish Woman in America* is a valuable contribution to the history of Jews and women in America.

ROBERT A. ROCKAWAY
*Tel-Aviv University*

KENNETH L. KUSMER. *A Ghetto Takes Shape: Black Cleveland, 1870–1930*. (Blacks in the New World.) Urbana: University of Illinois press. 1976. Pp. xiv, 305. $12.95.

Kenneth L. Kusmer has written the best book yet on the formation of a black urban ghetto. His analysis of Cleveland's Afro-American community between 1870 and 1930 not only extends work on other cities by Osofsky, Spear, Katzman, and the like but also elaborates—and sometimes revises—broader studies by Meier, Franklin, Frazier, and Spero and Harris. Kusmer utilizes an impressive volume of sources and explores every aspect of black life, including the experiences of women. Perhaps most importantly, he fits his analysis into a comparative context, showing how and why Cleveland duplicated or deviated from what happened elsewhere. This effort gives the reader a refined view of ghetto development and clarifies subtleties of the black urban experience.

Essentially, the book traces the effects of growth. Before the "Great Migration" that began in 1915, Cleveland's small black population grew slowly. Consequently, black institutions and leadership developed less rapidly than in larger cities and depended more heavily on white direction and support. At the same time, however, the socio-economic position of blacks deteriorated as whites tightened discriminatory control over employment and public accommodations. After 1915, the city's black population mushroomed, and the influx exacerbated racist trends. One result was acute segregation of blacks residentially, occupationally, and socially. Yet increasing isolation fostered the growth of new leaders and institutions which responded more directly to the ghetto's needs. By 1930 an expanded ghetto left Cleveland's blacks with intensified class stratification within their own community and a rising sense of cultural unity in response to external white prejudice.

The above summary seems obvious given what is already known. Yet Kusmer's thorough, facile study stretches far beyond these simplified generalizations. His sections on social disorganization and Garveyism, for example, are outstanding. Except for a somewhat dubious occupational index, that compares the occupational status of groups according to a rank-ordered scale, and a few

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strained passages, his book is convincing and readable. It stands as a tribute to the blend of urban and Afro-American history.

HOWARD P. CHUDACOFF
*Brown University*

CLARK C. SPENCE. *Territorial Politics and Government in Montana, 1864–89.* Urbana: University of Illinois Press. 1976. Pp. x, 327. $11.50.

This is a landmark book for the history of Montana and for the greatly neglected, distinctive American territorial system. Reflecting the highest standards of scholarship and written with a deft touch, it is one of the most useful works in its field. The author uses to full advantage the excellent case study which Montana provides. Its twenty-five year period was replete with extreme situations. The Democratic party dominated territorial politics, maintaining a running feud with the Republican administrations in Washington. Intraparty conflict was prolonged and violent. The northern "Unionists" and the southern "secesh" continued the Civil War for decades, and the partisan Montana press kept a thousand brush fires fully ignited.

Clark C. Spence follows chronologically the nine appointed governors, then considers the functions of the legislative and judicial branches, and finally turns to the difficult problems of financing the entire structure. Interwoven are questions common to all of the western territories such as the advance of the railroads, land policy, Indian policy, mineral taxation, and education. Meticulous research in newspapers, published proceedings, and personal correspondence has yielded short phrases and single-line comments which have been skillfully built into a mosaic narrative which gives the reader the feeling and flavor of the time.

Through no fault of the author, relatively little "new" information is contained in the book. It is invaluable, however, in that it gathers countless fragments of the entire period and process, organizes them clearly, and interprets objectively. Extensive citation will speed future studies immeasurably. The traditional negative attitude toward the territorial process continues to dominate, since little published research in its constructive results is available. The author often protests against the bombast of partisan conflict and repeatedly emphasizes that the territory was making progress and that the "carpetbag" appointed officers were perhaps as capable as a similar number elected in the state in a comparable period of time. But the narrative, pieced together from contemporary partisan sources, reflects the prevailing hypertension.

This study points again toward the need, long recognized in Montana, for monographs on the problems and personalities of the period and the positive development which did take place. It provides a framework of detail, organization, and interpretation which will be of the highest value in understanding and researching both the history of Montana Territory and the national territorial experience.

MERRILL G. BURLINGAME
*Montana State University*

WILLIAM W. FOWLER. *Woman on the American Frontier: A Valuable and Authentic History of the Heroism, Adventures, Privations, Captivities, Trials, and Noble Lives and Deaths of the "Pioneer Mothers of the Republic."* Reprint. Detroit: Gale Research Company. 1974. Pp. 527. $16.50.

Because of the recent interest in women's history which has produced several worthy monographs on frontier women, some readers may conclude that until now western women, like western blacks, were virtually invisible to Clio. This timely and quaintly-illustrated reprint of a nearly century-old work belies this assumption. Its author, William Worthington Fowler (1833–1881), lawyer, journalist, and stockbroker, pioneered his chosen field. Fowler lived in the Northeast and evidently gleaned most of his material from published works and brief personal observations. His purpose was to survey the long overlooked contributions of women to winning the West.

The author covers the westward movement through 250 years and a wide range of feminine activities—homemaking, nursing, overcoming wild beasts and hostile geography, traveling by every conveyance, converting, fighting, being captured by and escaping Indians, and battling outlaws. (Generally, Fowler stereotypes Indians as savages, while blacks are depicted more sympathetically.) Despite a uniform emphasis on action—which makes his yarns seem a combination of Parson Weems, Ned Buntline, and Walt Disney—Fowler avoids feminist movements. Ever eulogistic, his volume never refers to prostitution, female outlaws, or family failures. WASP ethnocentricity predictably pervades its pages.

Predating Frederick Jackson Turner, Fowler sees the frontier as remaking womanly traits. Many of his heroines, although "delicately reared in luxury," adjust remarkably to wilderness challenges, and he asserts that "each succeeding generation seemed to acquire new features of character, added force, and stronger virtues."

Every chapter begins with flowery generalizations on one aspect of women's work, followed by anecdotes about resourceful individuals.

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Though Fowler rarely uses footnotes and never cites pages, he occasionally refers to women's diaries, a field which he was early to mine. Unfortunately, the reprinter of this compilation has not added an interpretive introduction or modern footnoting. One finds a few errors in proper names, and statistics must be challenged, but most recognizable accounts are essentially valid. Despite a monotonously sentimental treatment and the obvious lack of insight into women's intellectual attainments, this detailed book reveals much of its era and recognizes women's unique force in new societies. Several episodes seem worth modern critical re-examination.

JOHN E. BAUR
*California State University, Northridge*

MARVIN E. KROEKER. *Great Plains Command: William B. Hazen in the Frontier West*. Norman: University of Oklahoma Press. 1976. Pp. xv, 216. $9.95.

William Babcock Hazen's career was not unlike that of a good many of his contemporaries in the military. An 1855 graduate of West Point, he was assigned to obscure posts in Oregon and Texas where he established a solid reputation for courage and ingenuity in the Indian war. With the onset of the Civil War, he rose rapidly in rank through distinguished service and at the war's end was a brevet major general. For the next fifteen years Hazen was again in the West, performing well in a variety of assignments. Most successful, perhaps, was Hazen's role in 1868–69 as General Sherman's special agent to settle the Southern Plains tribes on reservations in Indian territory. He also served as a temporary superintendent of Indian Affairs in 1870–71 and acquired a sympathetic understanding of the Indian problem. He also learned of fraud and cheating in the Indian Bureau and of payoffs in high places. As a result, he became a key figure in the impeachment of Secretary of War William Belknap, who was involved in the "sale" of post traderships.

Intelligent, able, honest, and energetic, Hazen had, nevertheless, a penchant for quarreling and airing his views in the public press. In so doing he made powerful enemies including Generals Sheridan and Custer and the financier, Jay Cooke. At his death in 1887, he was chief of the Signal Corps and in very real difficulty for "unduly criticizing his superior, Secretary of War Robert T. Lincoln."

Marvin Kroeker has made an interesting scholarly effort to rescue Hazen from obscurity. The odds are, however, that Hazen will remain relatively unknown and unappreciated despite the effort. Essentially Hazen was an excellent administrator—he did not die at the Little Bighorn, round up Chief Joseph or Geronimo, or otherwise engage in exploits that captured and held public attention.

WILLIAM H. LECKIE
*University of Toledo*

H. CRAIG MINER. *The Corporation and the Indian: Tribal Sovereignty and Industrial Civilization in Indian Territory, 1865–1907*. Columbia: University of Missouri Press. 1976. Pp. xv, 236. $11.00.

H. Craig Miner's detailed and penetrating account of the impact of corporations in Indian Territory is a valuable contribution to the historical literature of Indian-white relations in the Gilded Age. This excellent monograph describes the numerous ways in which American corporations worked to disrupt and diminish tribal sovereignty in Indian Territory between the close of the Civil War and Oklahoma's admission to statehood in 1907.

Miner shows how corporate money and intrigue exploited the division between tribal traditionals and progressives to gain lucrative, long-term leases and access to mineral rights in Indian Territory. Indian progressives, the great majority of them mixed-bloods, at first welcomed railroads in the years immediately following the Civil War. Other corporate interests followed in the wake of the railroads in the next four decades, each disrupting traditional practices, attracting new capital, and thereby further diminishing tribal sovereignty. Some Indian progressives openly collaborated with corporations, while others attempted to use the corporations to assure continued tribal sovereignty. Both collaboration and compromise failed, and loss of tribal autonomy followed. Only the full-bloods refused any accommodation with corporations or with government bureaucrats who urged compromise.

*The Corporation and the Indian* says more about the aggressive, exploitative practices of American corporations than it does about the dimunition of tribal sovereignty. That the aggressions of one contributed to the demise of the other is well established. What the author does reveal, especially in the last chapters of the book, is the extent of corporate intrigue and bickering over the lucrative spoils to be reaped in Indian Territory.

If there is a weakness in this book, other than occasional lapses in style and syntax, it is the author's failure to recognize that the government never acted as a neutral agency in relations between Indians and those who coveted their land and resources. This was not an impartial or "broker" government giving a fair hearing to all sides. Government decisions always worked in opposition to tribal sovereignty; moreover, even those

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decisions that favored tribes were usually delaying actions and ultimately reversed. Imperial agents in Indian Territory came in many shapes and forms. Among them were congressmen, government bureaucrats, and officials from the Indian Office.

WILLIAM G. ROBBINS
*Oregon State University*

JOHN M. CARROLL, editor. *The Papers of the Order of Indian Wars*. Preface by GEORGE S. PAPPAS. Illustrations by LORENCE BJORKLUND. Fort Collins, Colo.: The Old Army Press. 1975. Pp. xxii, 287. $22.50.

During the late nineteenth and early twentieth centuries there existed a number of organizations that were interested in military history, strategy, and tactics. Professional organizations such as the United States Cavalry Association published journals that carried articles on these and other topics. While not a professional organization, the Order of Indian Wars was a society consisting largely of active and retired military officers who met to commemorate the accomplishments of the frontier army. In 1911 *The Proceedings of the Order of Indian Wars of the United States* was printed and subsequently included the speeches delivered at the annual meeting.

*The Papers of the Order of Indian Wars* is an unusual volume. John M. Carroll has brought together the constitution and bylaws, material from the *Proceedings*, speeches that were excluded from that publication, a financial report, and the membership roll. In a three page introduction he provides little information on the organization and reasons for its formation, although brief introductions to items that are reprinted include such unimportant information as attendance figures for annual banquets. The published items are also a mixed bag. Many are reminiscences about specific engagements that authors admit may include errors regarding details; one is an account of George Washington's role in the French and Indian War; some such as Charles Gatewood's account of the surrender of Geronimo have been previously used by scholars.

Although information about the Order of Indian Wars might have made some contribution, this book is of little value, for few of the papers are of any significance. Perhaps the best part of this book is the illustrations by Lorence Bjorklund.

RICHARD N. ELLIS
*University of New Mexico*

NECAH STEWART FURMAN. *Walter Prescott Webb: His Life and Impact*. Albuquerque: University of New Mexico Press. 1976. Pp. xiv, 222. $12.00.

KENNETH R. PHILP and ELLIOTT WEST, editors. *The Walter Prescott Webb Memorial Lectures: Essays on Walter Prescott Webb*. With an introduction by RAY ALLEN BILLINGTON. Foreword by JUBAL R. PARTEN. Austin: University of Texas Press. 1976. Pp. xxi, 123. $7.95.

Here are two volumes that historians of the frontier will value highly, an excellent biography of Walter Prescott Webb by a new scholar in the profession, Necah S. Furman, and a series of essays on Webb coming out of memorial lectures at the University of Texas at Arlington. Furman's book, an extremely readable, well-documented biography telescoped into 222 pages, includes notes, bibliography, index, and illustrations; it gives us for the first time an overview of Webb's life as a professor, writer, and man. The Webb memorial lecture volume, with a perceptive introduction by Ray A. Billington, includes essays by Joe B. Frantz (Webb and the South); W. Turrentine Jackson (Australians and the Comparative Frontier—with an appraisal of Webb's comparative frontier concepts); W. Eugene Hollon (Webb and the Arid West); George Wolfskill (Webb's Great Frontier Hypothesis and International Law); and Walter Rundell, Jr. (Webb the Schoolteacher). These essays are a rich addition to Necah Furman's study of Webb's life, a story that in itself is a part of the pioneer hardship and legend of modern Texas history.

Furman's book begins with a long look at Webb's formative years as a Texas farm boy whose life was changed by the kindhearted New York benefactor, William E. Hinds. It was Hinds' generosity that enabled Webb to begin a school-teaching career and complete his undergraduate education at the University of Texas at Austin. Furman perceptively tells the involved story of Webb's romance with his future wife, Jane, the early source of his motivation and inspiration. Then, following his marriage and appointment as an instructor at Texas, Furman begins a series of topical chapters: on Webb's failure as a doctoral candidate at the University of Chicago; on his role in literary controversies; on his ideas on teaching, administration, and the defense of academic freedom; and on Webb's response to encroachments upon the land by those who sought only to exploit the soil and its treasures. Throughout we are treated to a lively account of the origins of his ideas and the genesis of his most influential books (*The Texas Rangers*, *The Great Plains*, *The Great Frontier*), as well as his lesser-known works (*Divided We Stand; Flat Top, A Story of Modern Ranching; More Water for Texas; History as High Adventure*). What is particularly stimulating in Furman's appraisal is that she sees Webb as a modern man who used history as a guide to understanding problems of the future. For

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instance, in the boom theory set forth in the *Great Frontier* and in the long series of articles and books on environmental problems concerning the Great Plains and the fragile desert ecology, Furman argues that Webb turned his eye on societal problems of growth. W. Eugene Hollon's excellent essay in the Webb memorial lectures also states that Webb was ahead of environmentalists in understanding the impending disaster that faces the arid West from overdevelopment and waste of water.

The authors in both volumes have probed the original sources and have given us new insights. Walter Rundell's penetrating piece on Webb as a schoolteacher shows that there is room for still another, more detailed biographical study. What this reviewer would like to see, however, is a more critical evaluation of the man. For though he gave us much, there are, as Furman suggests, overtones of Anglo-Saxonism and regional shortsightedness in many of his writings.

WILBUR R. JACOBS
*University of California, Santa Barbara*

KENTON J. CLYMER. *John Hay: The Gentleman as Diplomat*. Ann Arbor: University of Michigan Press. 1975. Pp. ix, 314. $15.00.

Believing that John Hay deserves more attention than he has recently received, Kenton Clymer attempts in 211 pages to examine Hay's thought, since those ideas "developed primarily in the domestic context, affected his thinking and action in the area of foreign affairs." He begins with Hay's literary and nonpolitical friendships, and then analyzes his economic and social theories, drawn mainly from the fictional *Bread-winners*, and his notions of race, allegedly a key to his diplomacy. Subsequent chapters treat Hay's views on international questions before and after he went as ambassador to England in 1897 and his response, on becoming secretary of state, to the annexation of the Philippines and the ensuing insurrection. The last four chapters are devoted to China, to the Boer War and the Alaskan boundary dispute, to the Hay-Pauncefote Treaties, and to relations with President Roosevelt. The result is a creditable first book in which several weaknesses, however, detract from the strong points.

The author examined over sixty collections of personal papers and unprinted records in the State Department. The Hay letters at Brown, the Schurman Commission correspondence in the National Archives, and the manuscript of *The Bread-winners* at Harvard yielded the most new materials. Hay's capacity for maintaining friendships is evident throughout. His antagonism toward the Boers and the Filipino rebels shows more clearly than in earlier works. While stressing Hay's elitism and snobbery, Clymer does not mistake flippancy in private notes for racism. His judgments on Hay are balanced and fair. The documentation is full; the bibliographical apparatus is helpful; the style is clear, though not exciting.

Still, one can question whether the book was properly conceived or perfectly executed. Hay is not a forgotten figure, and his views on social, economic, and racial issues do not warrant the space they receive. The subtitle tells nothing; it could be used for most of Hay's immediate predecessors and successors. The text, too condensed on some topics and too liberally sprinkled with qualifiers such as "perhaps," offers little new to specialists. Clymer does not reveal how Hay ran his department or dealt with subordinates. He does not assess the important influence on Hay of William McKinley or Elihu Root; in one key episode, the cable to the peace commissioners in Paris in October 1898, he is badly confused. Also missing is a needed probe of Hay's difficulties with the Senate, and here the research is at fault. Had the author used to better advantage the Henry Cabot Lodge Papers, he would have discovered a journal (cited by John A. Garraty in 1953) in which the Senator made notes on meetings with Hay and on proceedings in the Foreign Relations Committee (of which he was not then chairman, as Clymer states) during the battle over the first Hay-Pauncefote Treaty. Without intending to be unfair, Clymer consistently quotes Hay's attacks on his critics without giving the latter equal time.

Although Tyler Dennett's 1933 biography is equally flawed on certain foreign policy matters, it remains—except where the passage of four decades has provided better perspective and additional sources—the more sensitive, imaginative, and revealing study of a complex man.

RICHARD W. LEOPOLD
*Northwestern University*

DAVID F. BURG. *Chicago's White City of 1893*. Lexington: University Press of Kentucky. 1976. Pp. xv, 381. $17.00.

The World's Columbian Exposition held in Chicago in 1893 was seen by contemporaries, as by later historians, as a crucial event in American cultural history. The monumental buildings of the White City, the picturesque exotica of the Midway Plaisance, the innumerable categories of exhibits from around the world, and the international "congresses" for the discussion of weighty topics, seemed, in many ways, to mirror the values of the nineteenth century and, in other ways, to prefigure those of the twentieth.

The controversial development and legacy of the

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fair's art, architecture, and physical planning have been described and analyzed in a variety of studies, but less attention has been paid to the exhibits and congresses that took place inside the buildings. In giving approximately equal attention to all aspects of the Chicago fair, David Burg has attempted a long overdue synthesis. The results, however, are disappointing since the book is marred by both conceptual and methodological flaws.

Burg's two introductory chapters are relatively competent, uncontroversial surveys of American and Chicago culture in the late nineteenth century. Yet, much of his treatment of architecture and planning, from the Chicago School to the fair's Beaux-Arts Neoclassicism, is naive and ill-informed. (For example, the planner Jane Jacobs does *not* regard, as Burg suggests without critical comment, the City Beautiful movement "as simply another form of Ebenezer Howard's Garden City concept.") Burg's chapters on exhibits and congresses, especially those on religion, history, and the role of women, are, by comparison, useful synopses. His concluding chapter, however, on the legacy of the fair, is redundant and unfocused.

There is, inexplicably, no bibliography, but the book's eight hundred footnotes cite only published sources. How, in attempting a study of this magnitude, could the author ignore the surprisingly large number of unpublished dissertations on the fair and the rich manuscript resources at the Chicago Historical Society, the Art Institute of Chicago, and the Library of Congress? He made no attempt, apparently, to tap the Olmsted and the Burnham papers.

More disturbing than Burg's sole dependence on published sources, however, is his unimaginative treatment of them. He seems to have had no central guiding theses or overall points of view and few personal criteria by which to validate his sources. He frequently seems to subscribe to the views of whomever he is citing, no matter how contradictory such sources may be. And unfortunately, Burg's few attempts at personal value judgments result in confusing or simplistic platitudes ("It is possible that many people found the Midway to be their fondest recollection of the exposition, but that seems unlikely" [p. 336]; "Had the spirit of the parliament [of religions] been fulfilled, some of the horror of the twentieth century might have been prevented" [pp. 329–30].)

Vast portions of the text are direct quotations and large numbers of these are from secondary sources. Many of them, furthermore, contain the most routine information, which should have been paraphrased. Frequently such quoted passages are dropped into the text without sufficient transition and without textual identification of who is doing the speaking. This is scissors-and-paste history with a vengeance.

The "definitive" history of the Chicago fair is still to be written.

THOMAS S. HINES
*University of California, Los Angeles*

EDWARD R. KANTOWICZ. *Polish-American Politics in Chicago, 1888–1940.* Chicago: University of Chicago Press. 1975. Pp. xi, 260. $12.95.

In *Polish-American Politics in Chicago,* Edward R. Kantowicz examines the rise of Polish political leaders and their role in the immigrant community, campaign issues and voting patterns in Polish precincts, and relations between Polish politicians and the city's Democratic machines, which, until Anton Cermak's election as mayor in 1931, were generally Irish-dominated.

The book is based on interviews, Polish-language newspapers, manuscript collections, census reports, church and voting records, and registration lists. Unlike many recent studies of ethnic voting patterns, the text utilizes precinct voter registration lists (rather than census-tract data) to determine which precincts should be studied in order to isolate Polish voting patterns. The precinct lists are more reliable because, as the author accurately notes, "a census tract with a majority of Polish residents would not necessarily include a majority of Polish voters."

In addition to machine politics, Kantowicz discusses Polish reform politics and politicians and also relations between Poles and Jews both in the Old World and in Chicago. Unfortunately, he does not devote to these topics the attention they deserve. More surprising is the absence of any discussion of Polish involvement in, or connection with, prohibition-era crime. Was the Polish experience in this respect similar to that of Italian, Irish, and Jewish criminals, who forged mutually beneficial ties with local machine politicians? Did the Polish experience diverge significantly from this pattern—and if so, how? These shortcomings notwithstanding, Kantowicz has prepared a study that adds substantially to our understanding of the political achievements and failures of Chicago's largest new immigrant group.

HUMBERT S. NELLI
*University of Kentucky*

HERBERT G. GUTMAN. *Work, Culture, and Society in Industrializing America: Essays in American Working-Class and Social History.* New York: Alfred A. Knopf. 1976. Pp. xiv, 343. xvi. $12.50.

By common consent, Herbert G. Gutman is our

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leading social historian of American labor. A vigorous exponent of the English school of working-class history and a pioneering critic of the institutional approach to American labor history, Gutman has served as mentor and example to an entire generation of younger social historians. So it is an event when his scattered essays appear in collected form, and also an occasion for taking stock of the evolution of his scholarship. The persisting characteristics are careful and detailed research, an eye for the concrete and specific, and an exceptional sense for distinguishing what is known from what is conjectural (qualities that made Gutman so devastating a critic of *Time On the Cross*).

In the early essays (represented here by only two pieces) the organizing themes are modest and limited. Did the railroad strikes of 1873–74 foreshadow 1877? Why did two miners' strikes have different outcomes? In his second phase, Gutman strives to uncover the broader implications of his specific findings: the career of a black labor leader suggests a revision of the standard interpretation of race relations in the labor movement; certain episodes in Paterson, and the career of a socialist journalist living there, throw doubt on the conventional treatment of Gilded Age ideology and power structure; a study of the origins of Paterson manufacturers contributes to the debate on social mobility. Under the influence of E. P. Thompson, with his remarkable feeling for the worker as historical actor rather than passive object, Gutman has most recently been striving for a broader formulation of the social history of American workers. The first foray—"Protestantism and the American Labor Movement" (1966)—rather mechanically joins two themes that Thompson managed brilliantly in *The Making of the English Working Class*. The second essay, which gives the collection its title, is far more ambitious and innovative. It pursues Thompson's main theme of transition from preindustrial to industrial society, arguing, indeed, that the continuing repetition of this experience is central to the history of American workers, but, interestingly enough, shying away from Thompson's class analysis in favor of a formulation depending heavily on the anthropologist Clifford Geertz's distinction between culture and society. Gutman's current work is on the black family, but he is now returning to his earlier interests. How he builds on "Work, Culture and Society" will be of real significance for future historical scholarship on the American worker.

DAVID BRODY
*University of California, Davis*

JOHN ALEXANDER WILLIAMS. *West Virginia and the Captains of Industry*. Morgantown: West Virginia University Library. 1976. Pp. vii, 352. $11.00.

Between West Virginia's political beginnings during the Civil War era and the onset of a "troubled career in the twentieth century," both of which have attracted considerable attention, lay a third of a century in which the dominant forces were those associated with an emerging industrialism based principally upon coal, oil, and timber and the building of railroads. John Alexander Williams examines this period, from 1880 to 1913, through the business and political activities of four rich and powerful West Virginia industrialist-politicians, Democrats Johnson N. Camden and Henry Gassaway Davis and Republicans Stephen B. Elkins and Nathan B. Scott. Their mark upon the history of the state, he believes, has been no less significant than that of the statemakers themselves.

Underlying West Virginia politics during both the Democratic (1880–1897) and the Republican (1897–1913) regimes were struggles between traditional interests and the forces of the new industrialism. By 1900 Davis had overcome Agrarians, Redeemers, and the Kanawha Ring to emerge the most powerful figure in the Democratic Party. Elkins, his son-in-law and business associate, had wrested control of the Republican Party from older leaders such as Nathan Goff, Jr. and George W. Atkinson. Both introduced modern political methods and made generous use of money, retainers, and patronage.

In their efforts to assure benevolent federal and state policies, the industrialist-politicians endeavored, usually with success, to secure the election of friendly governors and legislators and to thwart or temper reformist or progressive tendencies of such men as Governors Emanuel Willis Wilson and William E. Glasscock and Congressman William L. Wilson. They themselves preferred the vantage point of the United States Senate, where one or more of the four sat for thirty-four of the years between 1871 and 1913. Williams has provided intimate details of their political operations, their Eastern industrial connections, the merging of their industrial empires with those of the great national leaders, and the origins of what he calls a *comprador* class in the state. He has illuminated an important aspect of West Virginia, and Appalachian, history and provided a corrective to much of the adulatory writing concerning the state's captains of industry.

OTIS K. RICE
*West Virginia Institute of Technology*

LOUIS GALAMBOS. *The Public Image of Big Business in America, 1880–1940: A Quantitative Study in Social Change*. With the assistance of BARBARA BARROW SPENCE. Baltimore: Johns Hopkins University Press. 1975. Pp. xii, 324.

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Starting with the assertion that the development of large organizations is a basic, but neglected aspect of recent American history, Louis Galambos analyzes changing middle-class attitudes about the corporation. He finds that, although middle-class Americans did express considerable hostility toward trusts and robber barons during the economic crisis of the 1890s, afterwards they accepted, at different paces and for various reasons, the depersonalized and bureaucratic view of corporations. The emergence of that accommodation to corporate culture, which Robert Wiebe earlier suggested, helps explain the conservatism of the New Deal reforms and public acceptance of broker-state politics favoring well-organized groups.

What makes this book distinctive is not the interpretations but the author's method, which he carefully explains. Dissatisfied with impressionistic generalizations about middle-class attitudes, Galambos focuses on specific occupational groups: southern and midwestern farmers, skilled workers, engineers, and Congregational ministers. For each group he selects one or more occupational magazines of wide circulation and systematically applies content analysis to nearly 9,000 items. From those quantifiable data come the evidence for his generalizations. Although the book makes unexciting reading, Galambos should be commended for dedication to a systematic method and for willingness to admit when new evidence supports older interpretations.

Despite the author's concern for methodology, the book is not entirely satisfying. It is a peculiar sample of the middle class that intentionally excludes the growing population of white-collar employees and that entirely neglects the small businessmen—perhaps because no magazines were available for content analysis. Even with his own selections Galambos does not provide a theory or model to explain the relationship between his magazine samples and the opinions of real people. Would a content analysis of the AAUP *Bulletin* be adequate for understanding attitudes of academics? Might there not be other quantifiable sources, such as public opinion polls during the 1930s, to verify some of these generalizations? In the end, what we learn from this book are not necessarily attitudes of real middle-class people but only the attitudes of some occupational magazines. The pity is that the generalizations and the author's intelligent commentary are enticing.

ALAN RAUCHER
*Wayne State University*

OTIS E. YOUNG, JR. *Black Powder and Hand Steel: Miners and Machines on the Old Western Frontier*. With the technical assistance of ROBERT LENON. Norman: University of Oklahoma Press. 1976. Pp. xii, 196. $9.95.

Otis E. Young, Jr. discusses the development of technology in frontier mines. The first major supplement to human muscles at the working face of a mine was blasting. Two men prepared the hole, one swinging the hammer while the other held the steel drill. The Bickford fuse was not an infallible way to touch off the unstable black powder, so the annoyance of a "hang fire" was minimized by lighting it just at the end of a work shift. Gradually the earlier technique was improved by Nobel's dynamite, electrical ignition, and the machine drill with a water feed to dampen the dust. Hauling material up a mine shaft was at first limited by the strength of hemp or manila rope; chain was no better. The solution was wire rope or a braided wire belt. Buckets and cages helped vertical extraction of material, but an incline skip was better. The Cornish beam engine, improved to meet American needs, was the best available device to pump out the undesired water that often seriously plagued operations.

The jackass or burro was the most important of all animals to the mining West for the lone prospector or the pack train. He required no grain, no picketing, no shoeing, no coddling; his good eyesight and intense curiosity made him an excellent sentry. Immigrant miners were apt to be Irishmen or Cornishmen, proud of their "Cousin Jack" heritage from generations of miners in Cornwall. The Cornishmen usually did the more skilled, but not necessarily easier or less dangerous, tasks. Underground work was supervised by the captain and the timber foreman; elsewhere the blacksmith and the hoisting-pumping engineers operated independently. On holidays a major attraction for prize money and gambling was likely to be the fifteen-minute drilling contests by two-man teams.

Otis E. Young, Jr., has already written excellently about techniques and equipment in *Western Mining* (1970). He is considerably more adept in handling geological and technological problems than any other historian who has tackled them. His present book, partly the result of recent research conducted at various old mining sites, supplies additional information about matters he has touched on earlier and is a valuable contribution to the field.

WILLIAM S. GREEVER
*University of Idaho*

WILLIAM GRAEBNER. *Coal-Mining Safety in the Progressive Period: The Political Economy of Reform*. Lexington: University Press of Kentucky, for the Organization of American Historians. 1976. Pp. xii, 244. $16.75.

William Graebner's analysis of the development and administration of national and state coal-mining safety legislation from 1900 to 1920 fully merits

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its selection by the Organization of American Historians as Frederick Jackson Turner Award winner for 1975. Consistently focused on the creation and evolving character of the national Bureau of Mines and on the effort to achieve uniformity in state mine safety legislation, Graebner places these reforms within the context of a competitive economy, technological change, the changing population mix in the mining industry, the conservation movement, an efficiency craze, a growing rate of industrial accidents, a national safety movement, an organizational revolution in federal-state relations, and a reforming spirit in politics—all part of the progressive era. A final chapter relating his findings to the broad range of Progressive reform historiography deserves the attention of all serious students of recent United States history.

As Gabriel Kolko and James Weinstein have done, the author places businessmen, coal operators like Aretas Fleming and Justus Collins, in the vanguard of the safety reform movement. The prime movers in the reform effort, however, were resourceful bureaucrats like Joseph A. Holmes and the state mine inspectors. The endangered miners and their union proved reluctant reformers indeed. Independence, greed, and ignorance led them to discount the workability of the dust theory of explosions and to resist the introduction of safer explosives and blasting methods and of increased supervision.

The research is thorough, the interpretation imaginative, but the author's logic falters on occasion. The same efficiency craze that fostered human resource conservation also produced a more sordid emphasis on productive but dangerous practices. It seems unlikely that nativism could inhibit life-saving practices in coal mining, but not prevent a successful safety movement in the steel industry, also a heavy employer of immigrant labor.

JOHN W. HEVENER
*Ohio State University, Lima*

LOUIS FILLER. *Appointment at Armageddon: Muckraking and Progressivism in the American Tradition.* (Contributions in American Studies, number 20.) Westport, Conn.: Greenwood Press. 1976. Pp. xiii, 476. $15.95.

Louis Filler's concern in this book is to restore vitality to the progressive tradition. "Even though progressivism is apparently established in history as a potent force in American life, it has fallen on strange days so far as its status in living affairs is concerned." Part of the problem of understanding progressivism, according to Filler, is that "recently the large hypotheses" dealing with the tradition "have given way to relatively small monographs exploiting groups of assembled papers and correspondence." He is most critical of New Left students of progressivism such as Gabriel Kolko, whom Filler sees as a narrow interpreter whose restricted vision has resulted in a distorted view of the period.

Filler offers his book as a contrast to this narrowness. In his description of the years 1900 to 1912, he writes about Theodore Roosevelt, Woodrow Wilson, and Robert LaFollette. He also discusses many urban and state progressive leaders, women and blacks, journalists and painters. The last half of the book is devoted to these years. The first half argues that a progressive tradition began in the Colonial period. The muckraking of Thomas Paine set the precedent for the many lesser nineteenth-century progressives such as Anne Royall and William Lyon Mackenzie whom Filler briefly discusses.

Dealing with such a long period and with so many personalities, Filler makes progressivism central to most American experience: "The syndrome of progressivism, then, meant first of all a feeling for expanding opportunities that might help an individual's private ambitions and that would feed his dreams and generally serve the politics of hope." He offers no more precise definition of progressivism than statements like these: "The one characteristic that all progressive laws had in common was their effort to stay abreast of the times," or that politicians "were supposed to be first of all practical, mediating, compromising—in short, progressive." Because he describes so much of American history as progressive, one is puzzled by Filler's enigmatic statements about the collapse of the progressive tradition during the First World War, a collapse which he does not explain. He makes the point that the Civil War, in contrast to World War I, revitalized the progressive tradition but he makes no effort to explain how or why.

DAVID W. NOBLE
*University of Minnesota, Twin Cities*

PETER H. ARGERSINGER. *Populism and Politics: William Alfred Peffer and the People's Party.* Lexington: University Press of Kentucky. 1974. Pp. xiii, 337. $15.50.

When Kansas Populists were considering whether to support Senator Peffer for a second term, Jerry Simpson was asked to evaluate the performance of his Populist colleague. In typical Simpsonese he replied that Peffer reminded him of a little bull on his ranch. Try as he would, the "critter" simply had failed to leave his mark on the herd. Although

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the analogy was reasonably accurate for that period, thanks to Peter Argersinger's diligent research and fixed frame of reference, Peffer most assuredly has left his mark. Peffer and Simpson were of course factional opponents; yet it is appropriate to recall Simpson's comment. In this study Peffer is clearly the hero and Simpson the villain.

*Populism and Politics* is not a full-fledged biographical treatment of Peffer, and surprisingly little new information emerges regarding the first Populist senator. Instead, Argersinger focuses on the man "primarily to illustrate the changes within Populism." More clearly, he utilizes Peffer to support his thesis that middle-of-the-road Pefferian Populism was genuine Populism, as opposed to the fusionist-Democratic politics of Weaver, Butler, Simpson, Breidenthal, Tauberneck *et al.*, which apparently was not Populism at all.

The book's major contribution is its treatment of Populism and politics at the national level, and, in spite of its occasional venture into "sophisticated" quantification (which, incidentally, largely reaffirms earlier and "less sophisticated" findings), the study is exceptionally readable.

Curiously, Argersinger concedes that "Populism included many different people with many divergent attitudes and ideas and had different factions dominant at different times." Yet the pervasive message implies that Pefferism was Populism; that only Populists of Republican antecedents were legitimate Populists. More seriously, Argersinger is too uncritical of a number of individuals who comprised the extreme antifusionist element or "lunatic fringe." The devotees of the "paranoid style" emerge as principled reformers; the Democrats and fusion Populists as unsavory opportunists. He concedes the influence of cultural and sectional prejudices and prohibitionist sentiments on Peffer, yet seemingly ignores the effect of these factors in determining the course of Peffer and more extreme antifusionists. At the same time the main proponents of the system that the Populists were seeking to alter (the Republicans) practically escape notice or censure.

It is unfortunate that the study is afflicted by this case of tunnel vision; it is otherwise an admirable monograph.

GENE CLANTON
*Washington State University*

WILLIAM R. HUTCHISON. *The Modernist Impulse in American Protestantism*. Cambridge: Harvard University Press. 1976. Pp. x, 347. $15.00.

The intellectual history of the later nineteenth century is among the most exciting yet most neglected areas in American history. *The Modernist Impulse in American Protestantism* makes a contribution toward understanding it that is solidly researched, well conceived, original, and persuasive.

William R. Hutchison defines modernism as including three components: a desire to adapt religion to modern culture, a belief that this adaptation is justified because religion is in fact an aspect of culture—or, as the Protestant modernists themselves would have put it, "God is immanent in human cultural development and revealed through it," and, finally, a postmillennial faith in progress entailing confidence that the modern world is indeed better than earlier societies and can thus provide the basis for a fuller, more accurate religion.

Although the first of these aspects is the best known, the second is by far the most interesting, and to it Hutchison devotes the most attention. He locates his Americans in a larger context of Western thought, identifying the British and German religious ideas that influenced them. If he had wanted to range more widely, he could also have related the emergence of modernism in religion to the emergence of cultural self-consciousness and cultural relativism in the social sciences. The great French sociologist Émile Durkheim, for example, developed a view of religion as an integral aspect of culture that bears a striking resemblance to the attitude of his contemporaries, Hutchison's Protestant modernizers.

Hutchison recovers the importance of a number of religious spokesmen of the age, among them David Swing of Chicago, tried for heresy and acquitted in 1874. He gives considerable space to the various critics of modernism, including Princeton theological conservatives, fundamentalists, the twentieth-century neo-orthodox, and those religious liberals who moved beyond the optimism associated with modernism. His judicious perspective combines appreciation with detachment and patronizes neither the modernists nor their adversaries.

In 1959 Hutchison published a definitive monograph on the Transcendentalist controversy of the eighteen-thirties and forties, admirable for its accurate reconstruction of the arguments on both sides. In that book he insisted that the conciliators who tried to mediate or quiet the dispute were really right. In his latest work, however, Hutchison shows no such disposition to prefer ecclesiastical calm over intellectual inquiry and debate.

DANIEL WALKER HOWE
*University of California, Los Angeles*

SHERNA GLUCK, editor. *From Parlor to Prison: Five American Suffragists Talk about Their Lives*. Foreword by KATHRYN KISH SKLAR. New York: Random House. 1976. Pp. x, 285. $3.95.

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*From Parlor to Prison* is based on transcripts of interviews with five women who had been active suffragists. The editor suggests that we see them as examples of the little people who, while they help to make history, are left out of written histories. This gives the women too little credit. None were leading suffragists chiefly because, with one exception, they were too young at the time to have qualified for high office. As a group they seem to have been exceptionally able and adventurous women. Even the most conventional of them, though primarily a middle-class, middle-aged housewife, ran an illegal birth-control clinic in St. Paul, Minnesota until her husband's death in 1917. Three of the women had successful careers, and a fourth spent many years in the trade union movement. Though exhibiting many differences and singularities, they tend to be more radical than the average suffragist, and probably also more resilient and flexible. Though proud of the suffrage struggle and their parts in it, a majority admire the feminist movement of today for going beyond political rights to the larger issues regarding women's place in society. On the other hand, all who address the point disapprove of the prejudice against men displayed by some ultrafeminists.

*From Parlor to Prison* is not aimed at professional scholars, but should be extremely useful in the classroom. It includes contemporary newspaper and magazine accounts that amplify comments made by the five women. Footnotes provide further information as required. But the book's great virtue is that most of the women led varied and interesting lives and talk well about them. This intelligently edited book has the additional merit of showing students by example how to create oral history on their own. I hope many are inspired by it to do so.

WILLIAM L. O'NEILL
*Rutgers University*

GEORGE S. MAY. *A Most Unique Machine: The Michigan Origins of the American Automobile Industry.* Grand Rapids: William B. Eerdmans Publishing Company. 1975. Pp. 408. $9.95.

In 1900 New England was the geographic center of automobile manufacturing in the United States. But by about 1909, southern Michigan was "by far the leading auto manufacturing center in the world." In this history of the inception and rise of automobile manufacturing in Michigan to 1909, George S. May intends "to suggest not only why Detroit, and in a larger sense Michigan, forged ahead of all other areas, but what is perhaps even more to the point, to answer such questions as why Ford succeeded and so many other Michigan mechanics with equal or superior abilities failed, and why Flint developed as a major automobile center while a city like Jackson did not."

May's concern is to provide a well-balanced, factually accurate account. And probably no one has more thoroughly or carefully researched the available sources. Consequently, May goes well beyond the conventional anecdotes and hearsay evidence to dispel numerous myths and correct often repeated factual errors about the early American automobile industry. For this he is to be commended. Serious students of automotive history, automobile buffs, and persons interested in Michigan history will undoubtedly find May's book a useful reference.

Nevertheless, this survey of personalities and events, however accurate, does not make a meaningful history for those of us who consider history either an interpretive art or a generalizing science. It lacks a strong overall thesis, new insights, and significant organizing hypotheses. It is also deadly dull. The reader must wade through too many lengthy, boring accounts of insignificant figures, companies, and events while central phenomena are given scanty treatment. May tells us more, for example, about the industry loser John Dolson or about Henry Ford's early career building racing cars than about the Ford Model N or Model T. Most important, May is so obsessed with detail and with pointing out the minor errors of his predecessors that he presents few new ideas and falls far short of providing a satisfactory explanation of the events he describes. After 334 pages of often irrelevant description, May devotes 13 pages to enumerating possible reasons for Michigan's hegemony. He concludes, "In the final analysis, no single factor was responsible for the origins of the auto industry in Michigan or for the way in which it then developed. Instead, an explanation must be sought through a study of a combination of factors." This tells us no more than we already knew, and such a study still needs to be done.

JAMES J. FLINK
*University of California, Irvine*

ANGELO ARA. *L'Austria-Ungheria nella politica americana durante la prima guerra mondiale.* (Storia, number 3.) Rome: Edizioni dell'Ateneo. 1973. Pp. 182. L. 3300.

This monograph by an Italian specialist in Austrian history analyzes the development of United States policy toward Austria-Hungary in 1917–18. It appears in the "Storia" series of monographs directed by Emilia Morelli, Ruggero Moscati, and Rosario Romeo of the University of Rome. The author has done his research in the National Archives, the Library of Congress, and Yale University's Sterling Library, but was unable to consult

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the important Series II (1917–1918) portion of the Public Papers of Woodrow Wilson, as these were then being reorganized in the Manuscript Section of the Library of Congress. Angelo Ara seems to be conversant with the literature in English, German, French, and Italian on his subject.

The author begins his monograph at the point where the 1958 Vanderbilt University dissertation of Gerald H. Davis, *The Diplomatic Relations between the United States and Austria, 1913–1917*, left off. Six chapters discuss the initial American evaluations of the Austrian problem; peace feelers after America's entry into the war; the maturation of American war aims with respect to Austria-Hungary; the equivocation regarding a separate peace; the development in the spring of 1918 of arguments in favor of dismembering the Habsburg Empire; and the American acceptance after July 1918 of the new national realities in the Danubian basin.

Ara recalls that Wilson remarked shortly after his inauguration in 1913 that it would be ironic if he had to concern himself primarily with foreign policy when his own interests and training lay in domestic policy. Yet that is precisely what happened—a fact which helps to explain the limitations and contradictions of Wilson's vision of international problems. Ara stresses that the Austro-Hungarian problem did not have independent significance in Wilson's thinking; rather it was simply one of several tactical elements on which the United States had to concentrate in order to weaken the German Empire. The author also argues that the postwar political and territorial shape of Danubian Europe resulted less from Wilson's Fourteen Points than from the specific actions of the subject peoples concerned during Austria-Hungary's collapse. Like Arno J. Mayer, Ara believes that the two major problems for American diplomats were the "containment" of Germany and (after November 1917) the halting of Bolshevik expansion, but he thinks that most policy-makers did not see before October 1918 the interrelationship between the shaping of the new political and economic structure of the Danubian area and the problem of checking the spread of Bolshevism.

Judging from remarks in the footnotes, Ara agrees with many of the contentions of Mayer, Victor S. Mamatey, Arthur S. Link, Arthur J. May, Harley Notter, and others regarding American policy in 1917–18. It is regrettable that he did not include a preface and conclusion in which he could have underscored whatever differences he has with their findings. Lengthy bibliographical footnotes substitute for a formal bibliography. The index is limited to names of persons mentioned.

CHARLES F. DELZELL
*Vanderbilt University*

JOHN PATRICK FINNEGAN. *Against the Specter of a Dragon: The Campaign for American Military Preparedness, 1914–1917*. (Contributions in Military History, number 7.) Westport, Conn.: Greenwood Press. 1975. Pp. ix, 253. $12.95.

John Patrick Finnegan describes the movement to increase and modernize the American military machine, mostly the army, during the years before American intervention into the First World War. He thus goes beyond John Garry Clifford's fine study of the Plattsburg Movement, although there is a chapter on the "Plattsburgers." Finnegan sees preparedness as a form of progressivism, an effort by military leaders, businessmen, and other public and private individuals to strengthen and invigorate the nation through discipline and enhancement of martial values. As diverse as the social reformers, the "military progressives" proposed changes ranging from a modest increase in the army to creation of a continental army (eliminating the much-maligned state militia or National Guard) to the grandest objective, universal military training. Even though preparedness became enormously popular, at one time even a national fad, it failed to accomplish all but the smallest objectives. Entry into war in 1917, of course, prompted vast rearmament, but it now became a much different proposition with different goals and leaders. The only contribution of the drive for peacetime preparedness, says the author, was psychological preparation for a national military effort.

Finnegan has told well what is indeed a complicated and many-sided story. His research—mostly in manuscript collections and periodical literature of that time—is thorough; the style is scholarly, a bit dry perhaps, but nonetheless clear. He documents American military weakness (many people felt the army could not defeat Mexico—which it almost had to fight—in 1916, let alone a major European foe or Japan), and describes the political thicket in which preparedness became entangled. Perhaps most important was the fact that supporters, who basically were isolationists thinking only in terms of defense, could identify only a "specter" and no real dragon threatening the United States.

The book's shortcomings stem from its narrow focus on participants in the preparedness movement. There is little effort to describe the impact over the months of the war; much of the time one scarcely can tell that a conflict is under way in Europe. The author gives President Woodrow Wilson's attitude only sketchy treatment. It would be interesting to know the belligerents' position on these strange goings-on in the United States. The book could be profitably larger and broader in

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scope. As it stands, however, it is a worthy effort, convincing, and altogether useful.

ROSS GREGORY
*Western Michigan University*

CAROL S. GRUBER. *Mars and Minerva: World War I and the Uses of the Higher Learning in America.* Baton Rouge: Louisiana State University Press. 1976. Pp. x, 293. $17.50.

In this discerning account of the academic intellectual's subservience to government during World War I, the author emphasizes the workings of academic politics. Insecurity about the social status of the profession and conceptions of the university as an organic part of the community led historians, among others, into the role of wartime propagandists acquiescing in attacks upon academic freedom and in the military conversion of their campuses.

Historians appear more vulnerable than social scientists in Carol S. Gruber's account, perhaps because advocates of the new history were then attempting to prove the relevance of their discipline. Their training in Germany increased their defensiveness rather than their dispassion or dedication to *Lehrfreiheit*. Ironically, those who condemned the German savants in 1914 for neglecting detatched criticism were to fall prey to the same disease within three years. Patriotism and the atmosphere of conformity only partially account for the appeal of such instruments of power as the National Board for Historical Service and the Committee on Public Information. The historians demonstrated a peculiar lack of self-awareness as they faced unresolved contradictions between their official duties and their dedication to truth.

When the author turns to the gross violations of academic freedom during the war the full significance of her extensive and new use of university presidents' and departmental archives from Columbia, Yale, Harvard, Wisconsin and Minnesota is evident. The account concerns the lack of commitment to academic freedom on the part of the academics themselves—the extent to which they were willing to use a man's patriotism or gentlemanly tact as the measure of his right to teach and do research. For this reason, this reader wishes that the careful examination of the internal Columbia University politics in the case of Professor James McKeen Cattell had been accompanied by materials from, and insights into, the response of Cattell's fellow psychologists. Similarly, the fine chapter on the Students Army Training Corps raises questions about the students themselves.

*Mars and Minerva* is itself outstanding testimony to the alternative that exists even for patriotic scholars during wartime. It demonstrates the author's "posture of critical independence from official policies and positions."

SONDRA HERMAN
*De Anza College*

ALFRED W. CROSBY, JR. *Epidemic and Peace, 1918.* Westport, Conn.: Greenwood Press. 1976. Pp. vi, 337. $17.50.

In this timely work, Alfred W. Crosby, Jr. deals with American phases of the great 1918–19 pandemic of Spanish influenza. He considers both the epidemic's civilian impact in selected communities and its devastating course through the armed forces near the end of the war. Several meaty chapters are devoted to the appalling statistics of the epidemic and to contemporary scientific research into the disease. A highly original chapter develops the hypothesis that the flu so debilitated both President Wilson and Colonel House as to materially alter the outcome of the Paris peace conference. And an imaginative final chapter seeks answers to the question of why most postwar American novelists and historians conspicuously blocked out this gigantic episode from their memories and from their subsequent writings.

This book deserves attention as the most thorough exploration to date of the pandemic's significance in American history. The author deserves plaudits for his provocative speculations and for a lively style which makes his solid scholarship eminently readable. His combination of medical, political, and social history is an effective one and should appeal both to professional historians and to general readers.

The volume is marred to some extent by its uneven organization, an uncertain chronology, and some signs of editorial haste. It is likely to annoy many readers with its loose generalizations, seeming assumption of superior wisdom, and journalese. While I have little argument over matters of substance, I wish the author had gone more deeply into the public health arrangements of the day, among other things to distinguish between the work of health leaders who were political hacks and those who were professionals. I also wish he had restrained a tendency to presentism, notably in dealing with the obvious frustrations of the scientific community in its confrontation with an elusive disease.

JAMES H. CASSEDY
*National Library of Medicine*

ROBERT K. MURRAY. *The 103rd Ballot: Democrats and the Disaster in Madison Square Garden.* New York: Harper and Row. 1976. Pp. xiv, 336. $10.95.

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Theodore White was born too late to write *The Making of the President, 1924*, since evidently White must cover a presidential campaign as a journalist in order to write a book about it later. Jim Bishop, undaunted by the lack of such immediacy, might one day give a try to *The Day the Democrats Divided*. I mention the two journalists not in order to categorize Murray's book with theirs, but in order to suggest that, like them, Murray has given microscopic attention to a moment of political history. Unlike them, he has written a history instead of a chronicle and has gone to the archives for documents rather than to campaign headquarters for a press conference. At the same time his book has all the eyewitness atmosphere, the revealing anecdotes, the hotel-lobby gossip, the telling trivia, the scraps of dialogue, and other useful devices that White uses when White is at his best.

Murray's primary concern is the sixteen days (June 24–July 10) in 1924, in Madison Square Garden, when the Democrats went at each other in a brawl that almost destroyed the party. The conflict between William Gibbs McAdoo and Al Smith for the nomination that finally went to John W. Davis, the interminable roll calls, the acrimony between the party's two factions (the Northeastern urbanities and the rural and small-town Southerners and Westerners), the heat and the bedlam on the floor and in the galleries – all this is familiar to students of the twenties. But Murray is the first professional scholar to take up these familiar elements and lay them out in what he rightly calls "an old fashioned descriptive and literary narrative." Quanto-historians will find no coefficients and correlations here. Social scientists will find every sentence strangely comprehensible, written in language that the delegates themselves could have understood.

To look closely at this 1924 Democratic convention, as Murray has done, is to peer into the troubled heart of the twenties. No other single event better mirrors the social and economic and ethnic and demographic and moral dilemmas then besetting the nation. Klansmen and fundamentalitists, Wets and Drys, Catholics and Protestants, city slickers and turd-kickers (only one pair of terms the factions swapped with each other), old stock Anglos and urban "ethnics"—all were at the convention. The party's representatives at Madison Square Garden were precisely that: representatives of just about every faction and interest in the Democracy if not in the whole republic. The most attractive quality about Murray's work is that in lucid prose and with persistent good sense he has focused intensely on the Democracy in the Garden and given the convention all the coverage it should ever need. (The reader soon feels like the delegates did: he wishes the thing would end.) At the same time he has shown how the convention was a microcosm of the larger sprawling society in the cities and the hinterlands of America in the twenties. For one man, in one book, to convey so much is admirable.

BURL NOGGLE
*Louisiana State University*

RICHARD WAYNE LYKES. *Higher Education and the United States Office of Education (1867–1953)*. Washington, D.C.: Bureau of Postsecondary Education, United States Office of Education. 1975. Pp. xii, 298.

The federal government today plays a crucial role in shaping the policies and programs of America's universities. This has not always been the case. While national concern for higher education was expressed very early in the history of the nation by means of land grants to states, the actual federal impact on college programs was slight. It became more important in 1862 with the passage of the first Morrill Act. Richard Wayne Lykes centers his attention on one phase of federal involvement with higher education following that act, namely, the activities carried on within the United States Bureau (later Office) of Education and, after 1911, the work of one of its components, the Division of Higher Education.

For a framework the author uses the administrations of the several Commissioners of Education and Chiefs of the Higher Education division. His account of their work is based largely on material taken from the annual reports of these agencies. This account, unfortunately, is not entirely satisfactory. We are given thumbnail sketches of education commissioners from Henry Barnard to Earl McGrath, but no picture in depth emerges from the narrative. We are told about the technical side of their administrative work, but very little about their social ideas or educational philosophy. The book as a whole is really not a work of historical analysis or synthesis. Events are not adequately explained in terms of the ideological currents or social and economic developments of the time. Half the volume is taken up with appendixes and bibliographies.

Despite its limitations, this work has some value as a reference tool. The compendious listing of all the publications on higher education sponsored by the Office of Education from 1867 to 1953 should prove useful. The biographical sketches of all professional staff members employed by the Division of Higher Education are a convenient summation. Moreover, Appendix H gives the full text of a suppressed report by staff member Kendrick Babcock in 1911. This was a pioneer attempt by a federal agency to do something like accreditation

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of colleges. It provoked such a storm of protest at the time that it was suppressed by orders of Presidents Taft and Wilson. It is good that the text of this controversial document has now been made readily available.

WILLIS RUDY
*Fairleigh Dickinson University*

ALBERT MARRIN. *Nicholas Murray Butler*. (Twayne's World Leaders Series.) Boston: Twayne Publishers. 1976. Pp. 226. $8.50.

Nicholas Murray Butler, for forty-three years president of Columbia University, was one of several amazing men at the turn of the century who simultaneously succeeded in building major universities and carrying on an active life in public affairs. Known neither for his classroom performance nor for distinguished scholarship, Butler had a knack for publicity, persuasion, and organization. He was a confidant of at least three presidents, several foreign heads of state, and numerous business and financial leaders. In the latter part of his career he was a guiding spirit of the Carnegie Endowment for International Peace. His opinions were sought by many, yet he was not an original or a prophetic thinker.

Nonetheless, Albert Marrin has chosen to focus this book almost exclusively on the ideas of Nicholas Murray Butler. (Unfortunately, Butler's personal papers are not yet available to researchers.) Marrin skillfully demonstrates that Butler held a coherent world view grounded in philosophical idealism. Academically trained at Columbia College and at German universities in formal philosophy, Butler had no difficulty in accommodating the ideas of Herbert Spencer and Charles Darwin. Following leads suggested by Immanuel Kant, Butler simply denied the claim of science to be the ultimate interpreter of nature and experience. To Butler, science and faith were autonomous realms of reality, each inaccessible to the other. From his belief in a transcendent moral world, Butler chided those American intellectuals who wavered toward a complete naturalism or tried to synthesize science and idealism.

Marrin argues that Butler, as a public man, was a "remarkably symptomatic figure" who reflected an important segment of American thinking in philosophy, education, politics, and international affairs. Marrin classifies Butler as a "liberal." Although the author gives the term some limits, it is often difficult to untangle Butler's ideas from those of other "progressives." No doubt many of the intellectuals of the era did share common presuppositions. Yet a sharply drawn comparison between the ideas of Butler and those of John Dewey, for example, would have lent greater clarity to this study. Then it would be more comprehensible why Butler opposed the "democratization" of the political processes, "progressive" education, the League of Nations, prohibition, and the child labor amendment.

Marrin's short book is the best available on Butler. What is now needed is a full-scale biography of the erstwhile Columbia president.

BENJAMIN G. RADER
*University of Nebraska, Lincoln*

THEODORE KORNWEIBEL, JR. *No Crystal Stair: Black Life and the* Messenger, *1917–1928*. (Contributions in Afro-American and African Studies, number 20.) Westport, Conn.: Greenwood Press. 1976. Pp. xvi, 306. $13.50.

The eleven-year existence of the *Messenger* covered a period of change in black America exceeded only by Reconstruction and the 1960s. The death of Booker T. Washington allowed for a flowering of new and more militant black voices, which were stimulated by the great northern migration and the heightened expectations resulting from the democratic rhetoric of World War I. The postwar Red Scare and black economic crisis dashed these expectations. Militancy became muted as black socialists accommodated to the existing union structure and black intellectuals found outlets in the Harlem Renaissance.

Theodore Korweibel's study views black life in the postwar years through the prism of the *Messenger*, a black monthly published by A. Philip Randolph and Chandler Owen, two of the "new crowd" of blacks vying for leadership in the post-Washington era. While the primary focus of the book is on the *Messenger*, our interest centers on Randolph, the most enduring black activist of the twentieth century. Through the pages of the *Messenger* we get a glimpse of Randolph's early activities. Starting as a "Journal of Scientific Radicalism" steering a socialist course, the magazine soon shifted to a more bourgeois appeal and finally ended as the organ of the Pullman porters' union. These moves were occasioned partly by the Red Scare, but more so by Randolph's perception of the nonideological outlook of blacks. Although he never renounced his socialist principles, Randolph toned down the nonracial aspects of his radicalism until he was barely distinguishable from his former bourgeois foes. Years later Randolph exhorted black leaders to "catch up with their followers." In the 1920s Randolph had to slow down for his followers.

*No Crystal Stair* is a well-written account of Randolph's early years. We see a man of iron character who at times acts culpably in his quest for lead-

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ership. While the tempering of his socialism can be attributed to astute leadership, Randolph's image is somewhat tarnished by his opposition to Du Bois and Garvey, which was based as much on personal ambition as on principle. The legend is, after all, human, and in revealing this, Kornweibel allows us a firmer base on which to analyze this giant of twentieth-century black life.

LEE FINKLE
*Indiana University–Purdue University, Indianapolis*

RAYMOND WOLTERS. *The New Negro on Campus: Black College Rebellions of the 1920s.* Princeton: Princeton University Press. 1975. Pp. viii, 370. $15.00.

"When the racial leaders of twenty years ago spoke of developing race-pride and stimulating race-consciousness, and of the desirability of race solidarity," wrote Alain Locke in his trenchant essay, *The New Negro*, in the 1920s, "they could not in any accurate degree have anticipated the abrupt feeling that has surged up and now pervades the awakened centers." As Raymond Wolters observes in this fine study, the "awakened centers" were the black colleges and universities, as well as black students in northern, white institutions.

By concentrating on the major black college rebellions, protests, and conflicts of the twenties, Wolters has neatly managed to encompass the various divergent forces operating within the black, middle-class intellectual and educational groups and their responsive actions on campuses. The central issue which kindled conflicts at such variegated institutions as Fisk, Howard, Hampton, Tuskegee, Florida A & M, Lincoln, Wilberforce—and a host of smaller, albeit sharp, encounters at NYU, Oberlin, Kansas, Harvard and others—were the paternalistic attitudes, industrial-vocational orientations, overly moralistic regulations and racist habits of many administrators, trustees, and faculty members. The younger generation pushed toward gaining control over the nature and direction of curricula and daily student activities, as well as toward an overall redressing of educational philosophy along the lines articulated by W. E. B. Du Bois. In fact, Du Bois' role "looms so large," writes Wolters, "that it is not possible to comprehend the phenomenon without understanding his education thought."

In contrast, Booker T. Washington's concept of black education had become institutionalized and fixed in the minds of both white boards of trustees and a number of black college and university presidents. Opposed to these circumstances as well as to the commonly held Darwinian view of the role or "place" of blacks, dissident students, alumni, and faculty advanced an integrationist position. Unlike their counterparts in the Black Power sixties, the earlier protestors sought "to escape from the backwaters of American life and join the mainstream." Clearly, they wanted "in" to the system. Indeed, Wolters argues, their energies were more akin to those of the civil-rights activists of the 1950s and 60s in that both groups derived from and reflected white middle-class aspirations.

An occasional vague thought intrudes in this broadly inclusive work. It is hardly instructive, for example, to learn that "those who have enjoyed a measure of freedom are as likely to protest against restrictions as those who have been shackled with prohibitions." But these hardly mar the thoroughness of research, overall design, and execution which makes the work valuable for a deeper understanding of an important episode in black protest.

JOSEPH BOSKIN
*Boston University*

TONY MARTIN. *Race First: The Ideological and Organizational Struggles of Marcus Garvey and the Universal Negro Improvement Association.* (Contributions in Afro-American and African Studies, number 19.) Westport, Conn.: Greenwood Press. 1976. Pp. x, 421. $17.50.

The research for Tony Martin's new evaluation of the career of Marcus Garvey is quite extraordinary. Most notably, he has utilized the recently opened records of the English Colonial and Foreign Offices and of the American State and Justice Departments, which enabled him to document the consistent fear, hostility, and opposition of the British imperial authorities and the United States government. Nevertheless, despite the superb scholarly digging, this is an unsatisfactory book.

For one thing there is the mechanical topical organization that often arbitrarily groups together essentially unrelated issues, neglects to treat regional variations in Garvey's movement, and fails to describe change and development in either Garvey's thinking or the movement. Even more bothersome is the author's tendency to speculate as to motivation and causation on the basis of flimsy or nonexistent evidence and to base significant conclusions on such speculation. And most important, this is a filiopietistic volume that defends Garvey's dealings with white racists like the KKK and Senator Bilbo, offers a decidedly unfair and biased treatment of Garvey's black critics, and fails to support the author's claims that Garvey was a revolutionary nationalist of superb organizational ability, who can be classified as the greatest black figure of the twentieth century.

Indeed Martin's own evidence contradicts his

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conclusions on major points. His analysis of Garvey's nationalist ideology simply does not sustain the thesis that Garvey was a revolutionary; indeed Garvey's emphasis on business enterprise, correctly stressed by Martin, was anything but revolutionary. Although Martin chooses to sidestep the internal organizational schisms that beset the UNIA, the fact is that Garvey was essentially a charismatic leader lacking in organizational ability (as is demonstrated by the discussion of the mismanagement of the Black Star Line). Finally, the assertion that the "integrationist onslaught" of men like W. E. B. Du Bois and A. Philip Randolph was a major cause of Garvey's imprisonment and deportation is contradicted by what I deem to be the most important contribution of this volume—the description of the long-range campaign by the Justice Department against Garvey, extending back to 1919, long before his black critics marshalled their forces against him. In fact, not a scintilla of concrete proof is offered to demonstrate that the Justice Department or any other government agency was influenced in its actions by the agitation of black radicals like Du Bois and Randolph.

Garvey is a major figure, whose career calls for careful and rigorous analysis based on exactly the kind of thorough research that Martin has done. It is therefore unfortunate that the author has chosen to present us with a spirited vindication of the celebrated black nationalist, rather than with a truly scholarly monograph.

AUGUST MEIER
*Kent State University*

J. E. CHAMBERLIN. *The Harrowing of Eden: White Attitudes Toward Native Americans*. A Continuum Book. New York: Seabury Press. 1975. Pp. 248. $8.95.

In this occasionally brilliant, often frustrating little book, J. E. Chamberlin weaves his way through four centuries of Indian-white relations in Canada and the United States. The title of the book announces the theme, but the subtitle is misleading, for Chamberlin writes essentially about white policy toward native Americans rather than about white attitudes toward Indian people. The book has many insights on the elaborate ways in which European legal, religious, and environmentalist theories have been employed in order to dispossess native occupiers of their land. The discouraging conclusion is that even when the settlers operated from genuine humanitarianism rather than narrow self-interest they were incapable, either in Canada or the United States, of devising policies that were not devastating to the people they confronted and conquered. From the first expressions of Christian concern for Indian souls to the Proclamation of 1763 to the Dawes Land Allotment Act of 1887 to the Alaska Native Claims Settlement Act of 1971, Indian policies, as devised by white governments, have misunderstood Indian people and consistently failed to resolve the "Indian problem."

The book will not satisfy very many historians of Indian-white relations, for it assigns an almost entirely passive role to Indians. Also it is poor comparative history, failing to discuss even the elementary differences in religion, national background, settlement patterns, and demographic growth in the French Canadian and British American colonizing societies of the seventeenth and eighteenth centuries, and how these factors affected Euro-Indian relations and the development of Indian policy. It is also disconcertingly muddled and self-contradictory regarding the origins of Indian policy—whether economic interests or ideological constructs determined policy-making or how these factors intertwined. The book, in the end, is best read as a passionate statement of the tragic confrontation of colonizer and colonized by a former policy advisor to the Canadian Department of Indian and Northern Affairs, who, at the close of this jeremiad, admits that he, like so many of the historical figures he portrays, is "utterly unable" to provide answers to the question of how Eden might have been less harrowed or may in the future escape the final harrowing.

GARY B. NASH
*University of California, Los Angeles*

DONALD L. PARMAN. *The Navajos and the New Deal*. New Haven: Yale University Press. 1976. Pp. xiii, 316. $17.50.

One of the problems in writing the history of the American Indian is that the subject matter has been traditionally considered the province of western and frontier historians. As a result, very little has been done on Indians and Indian policy past that magic date of 1890 when the frontier ceased to exist. Recently, however, a small band of historians has begun to explore the development of federal policy in the twentieth century. Not surprisingly, their research has centered on the New Deal era when the charismatic and controversial John Collier attempted to reverse the assimilationist policies of the past. Picking up where an earlier study left the Navajos in 1935, Donald L. Parman continues their story to the outbreak of World War II. His contribution is both abundantly detailed and superbly organized.

The major goals of the Indian New Deal were the restoration of sovereignty to Indian tribal

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groups and the rehabilitation of their land. In the case of the Navajos, Commissioner Collier was forced to choose between these goals because of the Indians' inability to comprehend the necessity of stock reduction on their overgrazed reservation. As Parman demonstrates, Collier ultimately came down on the side of land rehabilitation and thereby destroyed his initial plans for tribal political organization. Ironically, the land conservation program stimulated the development of tribal consciousness and the formation of a tribal council capable of expressing Navajo sentiment. Thus, as often happens, the desired result was eventually accomplished for all the wrong reasons, and Collier, who deserved better, became one of the villains in Navajo folklore.

There is so much to praise in this book that it is perhaps unfair to single out specific contributions for special mention in a short review. But some mention should be made of the chapters dealing with the implementation of the soil conservation program and the role of Senator Dennis Chavez in blocking Collier's promised expansion of reservation boundaries in New Mexico. Parman is the first historian to utilize the records of the CCC-Indian Division and the first to exploit the Chavez papers. In so doing, he has at last made these complicated subjects intelligible.

Parman set two goals in his study. The first, "to tell the story of the tribe's experience as objectively as possible," is accomplished to perfection. The second, "to assess the philosophy of Indian affairs" of John Collier, is less successful. In pursuing it, however, Parman clearly defines a problem with which all students of this era will have to contend, namely, distinguishing between Collier's claims to success and his actual achievements. Despite diligence in interviewing many of the participants in the Navajo experiment and the use of previously untapped regional archive collections, Parman does not appear to have made full use of the basic documents in the National Archives, and his footnote citations to those he did use are sometimes incomplete. Nor, unfortunately, does he appear to have consulted the work of his fellow historians. No mention is made of the studies of Margaret Szasz, Kenneth Philp, or Graham Taylor.

LAWRENCE C. KELLY
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WARD ALAN MINGE. *Acoma: Pueblo in the Sky*. Albuquerque: University of New Mexico Press. 1976. Pp. xii, 180. $12.95.

This well-documented and ably written history of the Ácoma Pueblo, done with the support and approval of Ácoma leaders, makes an important contribution to Pueblo Indian history and to the history of relations between Spain, Mexico, the United States, and the Indians of the Southwest generally. Ward Alan Minge, because of his interest in the Spanish colonial and Mexican periods, his role as chairman of the New Mexico Public Records Commission, and the research he has done on Pueblo Indian Claims cases, is well qualified to present the more than four hundred-year history of the Ácomas from the time of Spanish contact in 1540 to the present.

When the religious leaders at Ácoma are ready, it would be most interesting to be able to include a fuller account of the pre-Spanish contact period based on the oral traditions of the people. The results of archeological research would also be helpful.

The author's account includes information concerning the political, economic, and social history of Ácoma that is often also applicable to the other New Mexican pueblos. The Ácomas have adjusted admirably to the programs and institutions that the three different nations attempted to apply to the Indians. Sometimes the Indian's responses brought severe reactions from representatives of the ruling sovereigns.

As we read of efforts to bring European-American religious, educational, and medical practices to these village-dwelling Indians, we are made aware of the possibility of using Minge's work as a case study in the application of policies and programs developed by representatives of Western civilization to non-Western societies. What has taken place here is of the same nature as the experimentation the United States was involved with in the Pacific, Asia, and the Middle East during the two decades following World War II.

The design of the book and the tasteful use of photographs that depict the physical setting, the people, their agricultural and grazing economy, and their arts and crafts enrich the reader's learning experience and add to his pleasure. The seventeenth-century map used as endsheets, the informative tables, and the useful appendixes, notes, bibliography, and index make the use of the book rewarding for scholars as well as the general reader. A contemporary map showing Ácoma in relation to existing pueblos as well as other communities and physical features of New Mexico would have been appreciated by the reviewer.

Readers with an interest in the American Indian or the American West and public as well as academic libraries will want to consider this book for their collections. The University of New Mexico Press is to be complimented on this handsome volume, and with today's price structure, $13 is a bargain.

S. LYMAN TYLER
*American West Center, University of Utah*

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ROBERT F. HIMMELBERG. *The Origins of the National Recovery Administration: Business, Government, and the Trade Association Issue, 1921–1933*. New York: Fordham University Press. 1976. Pp. 232. Cloth $20.00, paper $7.50.

How did the federal government respond to the domestic activities and political pressures of American trade associations from the end of World War I to the beginning of the New Deal? Robert F. Himmelberg digs deep into archival and manuscript sources and the business press to provide a detailed answer to this question, making particularly full use of the Justice and Commerce Department records and the Hoover papers. The response, he concludes, like the pressures themselves, varied considerably under each administration.

The Harding administration pursued a contradictory policy. While Commerce Secretary Hoover generally encouraged associational activity, Attorney General Daugherty, responding to the political and economic unrest of the immediate postwar years, placed the entire trade association movement on the defensive with a series of suits against open price experiments. Hoover ultimately persuaded Daugherty, however, to compromise so as to allow the Commerce Department to provide statistical information to business groups. A more liberal dispensation occurred under the Coolidge administration, and especially under Antitrust Division chief William J. Donovan (of later OSS fame), who actively sought to expand associational rights. Hoover proved comparatively circumspect in this more pliant atmosphere, partly for fear of personal criticism, but more important, in Himmelberg's view, because of his commitment to the ideal of "competition as the organizing principle of the economy."

The implications of this commitment became even clearer, Himmelberg argues, when, as president, Hoover confronted rising demands for antitrust revision in the depression years. Hoover encouraged production stabilization in the oil, coal, and timber industries, to be sure, and generally renewed his efforts at voluntary trade cooperation. More characteristic in Himmelberg's view, however, was the Justice Department's decision to abolish the previous practice of granting informal approval to trade association proposals, to prosecute several trade groups, and to deny mounting demands among less profitable industries for antitrust revision. But the quest for a radical revision of antitrust practice awaited the New Deal; in the few weeks it took to draft the National Industrial Recovery Act, Roosevelt conceded a latitude on antitrust to business associations which Republican administrations had denied them for a decade.

In some respects Himmelberg does himself a disservice in casting his work as a prelude to NIRA, for nine-tenths of the book are not about NIRA's immediate origins at all. In his final chapter Himmelberg usefully amplifies the specific role business politicians played in the drafting process, and so supplements the accounts in Louis Galambos' *Competition and Cooperation* and Ellis Hawley's *The New Deal and the Problem of Monopoly*. And his general theme, that interwar lobbying by association leaders shaped the general administrative context for NIRA, if rather forced, is fair enough. But the book's major interest lies in the fate of antitrust in the pre-New Deal period, and in Herbert Hoover's role above all, and here Himmelberg could have been more explicit about situating his study in the ongoing revision of Hoover's career, along the lines, perhaps, of his essay in *Herbert Hoover and the Crisis of American Capitalism* (1973). The basic implication of this study seems to be that Hoover, for all of his recently discovered commitment to corporate thought and action, remained enamored of a competitive ideal which, if hardly committing him to the ancient version of laissez faire, still tempered his enthusiasm for combinations of either a private or public variety.

ROBERT D. CUFF
*York University, Toronto*

VICTOR G. REUTHER. *The Brothers Reuther and the Story of the UAW: A Memoir*. Boston: Houghton Mifflin Company. 1976. Pp. xiv, 523. $16.95.

Partly biography, partly memoir, and partly history, Victor G. Reuther's *The Brothers Reuther* is not an entirely satisfying book. It is scarcely *The Story of the UAW*, as the subtitle implies; rather it is, as Victor Reuther writes in the foreword, "a very personal book that would relate the experiences of three brothers and a family bound together by common hopes and aspirations and tempered by common suffering and tragedy." Without footnotes, bibliography, or other conventional scholarly paraphernalia, the book will be of limited use to labor historians.

Yet *The Brothers Reuther* is enormously revealing of the family background and personality that produced Walter Reuther. Indeed the finest passages in the book concern the relationships between the Reuther children and their parents and between the three brothers and their own families and the values that motivated the entire Reuther clan. In the opening pages of the book we see clearly how the Reuther brothers were shaped in childhood and adolescence by parents who stressed respectability, sobriety, individual achievement, and also Christian Socialism. The Reuthers appear driven by Weber's Protestant ethic, yet remain a family

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that cherished the nonmaterial, commonwealth characteristics which R. H. Tawney associated with the first generation of Protestants. As Valentine Reuther once wrote to his son Victor, "success in life should not be measured in terms of fortunes made, wealth accumulated, nor dollars saved, but rather in service rendered, in good deeds done, in friends made. . . ."

That advice was followed by all the Reuther boys, especially Walter, who in a high school essay wrote that "the thing that really counts is not how long we live, nor how hard we work, but what we accomplish. . . . By the wasting of your Time, you do yourself the greatest crime." The Reuther brothers never wasted time, always worked hard, and accomplished much. How hard they worked as labor leaders and how much they accomplished is relentlessly driven home by Victor Reuther, who can find no fault with his own or his brothers' policies.

When Victor shifts away from family memoir and comments on historical events, his judgments are less reliable, perhaps because he is too eager to rationalize the behavior of his brother, Walter. Walter Reuther can do no wrong, his labor opponents no right. When the Reuthers cooperate with the CIA to contain communism overseas, it is justified; when George Meany does the same, it is condemned. Victor considers the UAW's secession from the AFL-CIO necessary and justified morally and politically; yet he never explains, or justifies, why the UAW then united in the Alliance for Labor Action with one of the most corrupt and least democratic unions, the Teamsters.

The book includes superb photographs of and by the Reuther family and a loving account of Victor and Walter's European bicycle journey of 1932–34 during which they witnessed Hitler's rise to power and worked at the Gorky auto plant in Soviet Russia. For those eager to understand the character of the Reuther brothers and why Walter differed from most of his labor-movement contemporaries, this book is essential. Those who want to know more about the UAW, the labor upheavals of the 1930s, and trade unionism in the post-World War II era will still have to look elsewhere.

MELVYN DUBOFSKY
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GEORGE MARTIN. *Madam Secretary: Frances Perkins*. Boston: Houghton Mifflin Company. 1976. Pp. xv, 389. $16.95.

In a review some twenty years ago, Frances Perkins gently reproved this writer for associating New York's industrialists with indiscriminate opposition to Governor Franklin D. Roosevelt's social-welfare programs. And she was right, as she was with so many positions taken in her lifetime—as inquisitive college student, settlement house worker, colleague of Florence Kelley, pragmatic member of Eugene Debs' Socialist party, educator of Robert F. Wagner, Sr. and Alfred E. Smith after the Triangle Fire, and firm but soft-spoken governmental official.

She impressed her chauvinist colleagues in a hostile, male-dominated world as a "quiet orderly woman who 'did not buzz-buzz all the time or butt in where she was not wanted but who could be trusted not to be an embarrassment'." And she was fully trusted by Governors Smith and Roosevelt, despite their parting of the ways in 1932. The only individual to serve FDR as a cabinet member from the moment he became governor in 1929 until his death while president sixteen years later was Perkins.

Although organized labor opposed her appointment as a cabinet member, because she was a woman and not a union member, Perkins served the workers of this nation effectively and knowledgeably. Her deeply religious tenets affronted by the terrible shortcomings of an industrialized society, she devoted her life to educating the oppressed about their rights, to attaining women's suffrage, and to eradicating, through incremental legislation, the evils of child labor, the exploitation of consumers and workers, and the disastrous impact of unemployment upon human beings. During the Depression years of the early New Deal, Perkins finally achieved her lifetime goals of public works, minimum wages, maximum hours, unemployment and old-age insurance, abolition of child labor, creation of a federal employment service, federal aid to the states for direct relief, and the promotion of labor laws.

Her life in Washington as secretary of labor was not easy, for she had to cope with political and antifeminist opposition from key newspapers. Nor did she always have support from FDR. Somewhat typical was her insistence on the preservation of due process for aliens, who fell within her department's jurisdiction. And when, after the 1934 longshoremen's strike on the West Coast, Australian-born labor leader Harry Bridges was threatened with deportation because of unproven charges of advocacy of the violent overthrow of the government, it was Perkins who insisted upon postponing hearings until the U.S. Supreme Court had ruled in the forthcoming Strecker case.

In contrast to too many others, Perkins was a tower of strength against the "abuse of executive power," for she insisted that as an administrative official she "had a duty to take judicial decisions into account 'before subjecting other persons to punitive action and putting them to the expense of

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a prolonged and hard defense of themselves.' " Because of her belief that privacy was the basis of liberty, she was embroiled in continuing arguments with FBI Director, J. Edgar Hoover, opposing the fingerprinting of aliens, and then of all citizens. And she opposed, although privately, FDR's incarceration of 110,000 Pacific Coast residents of Japanese ancestry in concentration camps during World War II.

George Martin has given us a beautiful, sensitive, exceedingly well-written, analytical biography of the first woman member of a United States cabinet. And thanks to the author, we have much to learn from the ethics, morals, and total dedication of a pioneering woman who was actively committed to a more just and humane society.

BERNARD BELLUSH,
*City College,*
*City University of New York*

HAROLD JOSEPHSON. *James T. Shotwell and the Rise of Internationalism in America.* Rutherford, N.J.: Fairleigh Dickinson University Press. 1975. Pp. 330. $14.50.

The quest for examples of moments and visions of alternatives to aggressive and belligerent foreign relations has never been so intensive among historians as it is now. Examples of wars that did not happen despite critical tensions, studies of successful arbitration and negotiation, biographies of men and women who crusaded for peace and against militarism are becoming the height of historical fashion.

This life of James T. Shotwell, scholar and activist, who devoted his career to "the Rise of Internationalism in America," seems at first just such an important study. Shotwell was one of the few internationalists whose active life spanned both world wars and several of the most frigid years of the cold war. His personal quest for nonviolent alternatives in foreign affairs was ambitious and constant. In 1918 he was a member of The Inquiry. He was president of the League of Nations Association (1935–39), director of the Division of Economics and History of the Carnegie Endowment for International Peace (1924–48), and prominent in the San Francisco meeting of the United Nations. Harold Josephson scans all his most significant works. He also covers Shotwell's long academic career and analyzes his scholarly and polemical writing (very much the best parts of the book). He guides us through the labyrinths of power where academic ideas regarding nonviolent human relations have had such a spotty impact—causing Shotwell's optimism to flag utterly on occasion, as it did when he wrote *On The Rim of the Abyss* (1936) wherein he noted, "The race with catastrophe may be lost."

But Josephson's book does not adequately consider Shotwell's vision. The author criticizes Shotwell's missed opportunities, blunted connections, and truncated activities. Although he has written a study of failure filled with facts that just rip along, nowhere does he provide the context or the insights we need to assess the twentieth-century movements for peace or internationalism, or even war. Many of these movements remain misunderstood because historians persist in naming movements for war, or at least mobilization for war, peace movements. Josephson's chapter "The Historians Mobilize" exemplifies that problem. He writes that Shotwell "understood, moreover, the necessity of entering [World War I]. Even before the President had come out for intervention Shotwell had taken sides with . . . the Allies." Then Shotwell urged "unity in the historical profession" to establish "a correct public opinion" and was one of the founders of the National Board for Historical Service, to be associated with George Creel's Committee on Public Information.

Ignoring the nature and complexities of the World War I peace movement entirely, Josephson grants such new, vigorous, and lasting organizations as the Fellowship of Reconciliation, the Woman's Peace Party (renamed the Women's International League for Peace and Freedom), the War Resister's League, and the American Union Against Militarism (the parent organization of the American Civil Liberties Union) less than one paragraph. He then examines the bank accounts of Shotwell's organizations and judges them significant, significant enough to conclude that they were "by far" the "most prominent and wealthy peace organizations."

Nowhere does Josephson analyze Shotwell's notions of imperialism or the meaning of his vision of "world community." As Gerald E. Markowitz has written, Shotwell considered imperialism without war "both possible and desirable" (introduction to *On the Rim of the Abyss*, 1972 ed.). But Josephson does not discuss Shotwell's perceptions of the West's interests abroad, its need for markets and resources. Failing to deal with the limitations that set Shotwell apart from the activist peace movements spawned by the First World War, Josephson also fails to appreciate the value, notwithstanding those limitations, of Shotwell's long and active career. It was a career rooted in his awareness that war and conflict anywhere threatened peace and stability everywhere.

BLANCHE WIESEN COOK
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DAVID HOLBROOK CULBERT. *News for Everyman: Radio and Foreign Affairs in Thirties America*. Westport, Conn.: Greenwood Press. 1976. Pp. xvi, 238. $13.50.

This short book appraises the 1930s radio broadcasts of Boake Carter, H. V. Kaltenborn, Raymond Gram Swing, Elmer Davis, Fulton Lewis, Jr., and Edward R. Murrow. David Holbrook Culbert's conclusion that newcasters helped to create a climate of opinion favorable to an interventionist foreign policy seems well taken, and reminders of Kaltenborn's marathon broadcasts of the German-Czech crisis in September 1938 and of Murrow's "This is London" broadcasts are useful. Conclusions about the pre-eminent place of radio in combating isolationism and the "major" role of the newcasters in shaping public opinion are questionable, as is the view that radio created mass interest in foreign affairs after Munich, rather than that the growing crises generated interest in newscasts. And it is sheer exaggeration to say that one must "think in terms of radio, not newspapers" when discussing public opinion and foreign affairs in the 1930s, or that radio "made the news personal and immediate in a way that television never has been able to equal"—thus ignoring myriad telecasts from the McCarthy era through Watergate.

Culbert's criticisms of Carter for failing to see the American stake in the outcome of the wars in Europe and Asia and of Swing and Davis for being "tools" of the administration for willingness to risk intervention after May 1940 appear contradictory. More should have been said about whether the Roosevelt administration seriously sought to deport Carter, the decision by CBS to ban him after 1938, and how radio networks developed institutional policies with respect to constitutional—and competitive commercial—issues. Richer intellectual portraits might have emerged from more analysis of the relation between what newscasters said in the 1930s and what they wrote and did then and later. Finally, quotations of government officials' perfunctory notes to or about newscasters are uninstructive, while stylistic infelicities and innuendoes abound, for example, about the "many avowed Communists" in the 1941 Brooklyn College graduating class, Murrow's youthful tendency to embellish his vita that helped him to become a CBS vice-president, and Davis' desire to suppress "wrong" opinions. In sum, although the book sheds light on 1930s newscasting, it is thin and occasionally pretentious.

ARNOLD A. OFFNER
*Boston University*

HOWARD MCGAW SMYTH. *Secrets of the Fascist Era: How Uncle Sam Obtained Some of the Top-Level Documents of Mussolini's Period*. Carbondale: Southern Illinois University Press. 1975. Pp. xxi, 305. $15.00.

This book deals with six Italian documentary collections: Foreign Ministry records dispatched by the Italians themselves to their Lisbon legation on Mussolini's fall in 1943; Count Ciano's diaries and papers; Italian military records taken by the Germans to their *Aktensammelstelle Süd;* documents from the Fascist Ministry of Popular Culture; Mussolini's private papers; and some miscellaneous records of Mussolini, Graziani, and the army. Howard McGaw Smyth's objective is limited to "external criticism," namely, to establish the documents' provenance and authenticity and to describe their passage into Allied hands and their present availability. Only incidentally do the actual contents of these collections receive comment.

In outline the documents' story is not unfamiliar, but here the threads are drawn together and details displayed in authoritative fashion. Any worthwhile bibliography is necessarily comprehensive and meticulous. But in addition to these conventional qualities this book possesses a more unusual feature—in places it resembles a tale of international adventure and intrigue. Amid the confusion of war-torn Italy, 1943–45, the Italians were concerned to keep their most confidential material out of the hands of both Nazi occupiers and Allied invaders; for their part, Germans and Anglo-Americans launched a hunt for Italian documentation for purposes of "psychological warfare," that is, propaganda, as well as to bolster their own historical archives. In the welter of conflicting motives documents were lost, purloined, and hidden. In due course, some records were found behind partitions, copies of others were literally unearthed in a German rose garden, and yet others were seized by Italian partisans to appear later in fragments in *L'Unità*. To reconstruct the trail and fate of this material required real detective work, and the built-in excitement of the narrative has no need of the stimulus of a whimsical vernacular which is occasionally affected—witness the subtitle. Overall, though, this book is highly commended to all students of Fascist Italy on two counts: as an indispensable reference work, and as a fascinating account in its own right.

ALAN CASSELS
*McMaster University*

HERBERT F. YORK. *The Advisors: Oppenheimer, Teller, and the Superbomb*. San Francisco: W. H. Freeman. 1976. Pp. x, 175. $6.95.

In this small book Herbert F. York has given historians of the cold war a valuable monograph that

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will help them to evaluate President Truman's momentous decision in January 1950 to accelerate the development of the thermonuclear weapon. Specifically, York examines the recommendation of the prestigious General Advisory Committee to the Atomic Energy Commission favoring a delay in that decision so as to permit the United States to try once again to achieve agreement with the Soviet Union on international control of nuclear weapons. After examining the still scanty but growing public record of the course of nuclear weapon technology in both the United States and the Soviet Union during the 1950s, York concludes that the GAC's recommendations were based on what turned out to be remarkably accurate assumptions about the future course of weapon development in both countries. York also argues that acceptance of the GAC's advice would not have resulted in a dire blow to American security even if the Russians had continued to pursue the hydrogen bomb.

In presenting his evidence, York clarifies some important facts related to this question: 1.) the Soviet Union did not have a thermonuclear weapon in the American sense in August 1953 as both Russian and American officials suggested at the time, but only in November 1955; 2.) the critical concept which made the American thermonuclear test possible in November 1952 did not involve the use of a dry thermonuclear fuel; and 3.) there was a highly significant application (called "boosting") of hydrogen isotopes in fission weapons as well as in thermonuclear devices. Appreciation of these facts and their implications is essential for historians attempting to assess United States nuclear weapon strategy in the 1950s.

York's book is not history in the strict sense, and he does not claim to be an historian. But as one who has been a nuclear physicist, weapon designer, laboratory administrator, government official, and presidential advisor, York is well qualified to evaluate in a nonhistorical way the common assumption that Truman's decision saved the United States from nuclear domination by the Soviet Union. It is still hard to see how Truman could have decided otherwise in the historical context in which he acted, but York shows that the alternatives were more feasible than many could dare to hope at the time.

RICHARD G. HEWLETT
*U.S. Energy Research and Development Administration*

JOHN N. THOMAS. *Institute of Pacific Relations: Asian Scholars and American Politics*. Seattle: University of Washington Press. 1974. Pp. ix, 187. $8.95.

The relationship between scholarship and politics in a democracy has always been difficult and complex. There can be no pat or permanent solutions to the complicated issues and conflicting "rights and values" involved with respect to academic freedom versus governmental responsibilities, objectivity versus commitment, detachment versus involvement. The Vietnam War revived the democratic dilemmas regarding relevance, secrecy, and humane values in government and public life in America. This small but perceptive study by John N. Thomas seeks to analyze the impact of the anti-Communist "climate of opinion" during the McCarthy era on the Institute of Pacific Relations (IPR). Thomas also traces in some detail how the good work of the institute was lost sight of when Congress began "searching for the 'Reds' under every bed," how the IPR's image was tarnished, and how it had financial and other difficulties that led finally to its extinction in 1961.

Though the book does not purport to be a history of the IPR, it tells us some of the hitherto not-too-well-known details: Edward Carter's inability to restrain his friend Owen Lattimore; the latter's retrospective lapses and failures as the editor of *Pacific Affairs*; Frederick V. Field's inexplicable change of mind on resigning from the IPR; and William L. Holland's failure to take command of the situation once the accusing public exposure of the congressional hearings gained momentum.

After recounting and analyzing in detail the early rumblings of pro-Soviet and pro-Chinese Communist bias; the McCarthy charges, the Tydings and later the McCarran hearings; the resulting deterioration of genuine differences over alternative interpretations of Asian reality into those of rivals and adversaries and, subsequently, exaggerated into issues of security and loyalty and the drying up of the foundation finances, Thomas seeks to offer some conclusions and lessons with respect to the larger problem of congressional processes and scholarly groups in America. With good logic, Thomas argues that private organizations should have clear goals, that their leaders should have firm guidelines between the policy-making organs and the staff, and that the problems of regionalism in such groups should not be ignored. The history of the IPR, as Thomas clearly suggests, highlights the difficulty of maintaining a private organization that sponsors research in matters involving political controversy. In the ultimate analysis, the final safeguard for creativity in social science research must be sought in a country's public opinion and the good sense of its governmental institutions.

B. RAMESH BABU
*University of Bombay*

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COLE BLASIER. *The Hovering Giant: U.S. Responses to Revolutionary Change in Latin America.* (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1976. Pp. xix, 315. Cloth $15.95, paper $4.95.

Cole Blasier, a political scientist, has produced an important book on a subject which has been monopolized either by official apologias or left-wing condemnations. He has not only written an interpretive synthesis of United States reactions to Latin American revolutions from 1910 to 1961 (with an addendum for the post-1961 period), but he has built this around a detailed discussion of the various revolutions and the specific U.S. responses.

This study concentrates on four revolutions: Mexico (1910–1940), Guatemala (1944–1954), Bolivia (1943–1964), and Cuba (1956–1961). From the standpoint of the professional historian the presentation of the Mexican case is the most satisfying. This is owing to the fact that much more archival research has been done in the area, and Blasier had a richer body of secondary monographs to utilize. In addition, his own primary research involved both German and United States records and his material on Mexican-German relations adds a new dimension to our knowledge of the subject.

Blasier sees three stages in United States policy toward revolutionary developments. Stage one is the insurrection of the "rebel movements." Until the 1960s, he argues, U.S. policy involved some flexibility in all four cases. Stage two is characterized by "reformist governments," and Blasier states that U.S. policy was generally hostile. And he notes that the ultimate consequences of this opposition "were governments which took an even more independent line and created more serious problems for the United States than the reformist presidents had themselves" (p. 68). The third stage, labeled "revolutionary change," involved ultimate reconciliation in the cases of Mexico and Bolivia and armed confrontation in the cases of Guatemala and Cuba.

Blasier then analyzes various explanations for these reactions. Economic and bureaucratic factors are given due credit, but he rejects the arguments that United States policies were solely dictated either by capitalistic interests or by the internal governmental bargaining process. Instead, he argues that American leaders' perception of strategic interests was the basic element in all the responses and took precedence over the economic when the two were in conflict. The main criterion was ". . . could the Latin American revolutionary governments be counted on not to become pawns of a Great Power hostile to the United States?" (p. 216). But, Blasier points out that perceptions could be distorted by fears and misinformation.

A short review cannot do justice to the extensive analyses presented or to various minor points of disagreement between the author and the reviewer. For example, I do not agree in every respect with Blasier's discussion of Woodrow Wilson's Mexican policy. In the last analysis this is an honest, well-argued study which presents the complexities of U.S. responses to revolutions with a high degree of skill and fairness. Until the archives for the 1950s are opened, this book will stand as the best overall study of the subject.

ROBERT FREEMAN SMITH
*University of Toledo*

PHILIP J. JAFFE. *The Rise and Fall of American Communism.* Introduction by BERTRAM D. WOLFE. New York: Horizon Press. 1975. Pp. 236. $10.00.

JAMES WEINSTEIN. *Ambiguous Legacy: The Left in American Politics.* New York: New Viewpoints/Franklin Watts. 1975. Pp. xi, 179. $4.95.

Today the organized socialist political movement in the United States is weaker than it has been for the last seventy-five years, and James Weinstein informs us in his preface he is "concerned with the creation of a new socialist party in the United States—one that can build upon the experiences of the American left in this century." He provides us with a survey of socialist movements since 1900, focusing almost exclusively on the Socialist party, the Communist party, and the New Left; his purpose is to instruct those who share his socialist views about the lessons to be learned from American socialist history—what errors must be avoided, what successful tactics or ideas should be employed again.

There are problems inherent in a "lessons of history" approach, but on the whole Weinstein's book is a useful introduction. One who knows little about the subject would do well to begin with this volume. One can disagree, and I do, with several of his generalizations, such as that in the 1960s "the students at Berkeley and other elite schools" were "highly sensitive to their approaching proletarianization," but the book remains a solid condensation of recent American socialist history. For this and previous work he has researched widely and thought intensively. Sometimes Weinstein's erudition and interests cause him to devote disproportionate space to a topic. One wonders why, given his purpose, he budgeted about five percent of his text for a précis and critique of William Z. Foster's *Toward Soviet America* (1932) and allowed himself room for only an extremely sketchy history

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of the Communist party from the 1948 election to Khrushchev's "secret speech" of 1956 and its ramifications within the party.

Philip J. Jaffe's book is partly a history of the Communist party from 1930 to 1945, its most successful but most critical years, partly a biography of Earl Browder, who was the party's leader through most of this period, and partly a personal memoir. A good friend of Browder's who was very close to the party although not a member, Jaffe learned a great deal about the party's inner workings through conversations with Browder, particularly after 1945. Browder wanted Jaffe to write his biography, and Jaffe steadfastly resisted. He gave Jaffe hundreds of unpublished documents: letters, drafts of incomplete autobiographies, notes on interviews, and two messages from the Comintern that had come to Browder in code by shortwave radio. (These coded radio messages were received in September and October 1939, soon after the Stalin-Hitler pact; their content, a spelling out of the new Soviet line, is not as interesting or revealing as the method used to send them.) Jaffe's main contribution in this often fascinating, but sometimes distressingly repetitious, book is the new information he received from Browder's conversations and documents.

To this reviewer the most rewarding sections of the book are those about Browder and the author. Jaffe is an admirer of Browder—and was a "Browderite" at the fringes of the party—but he is certainly not an uncritical admirer. His friend, he concludes, "like Stalin, was a re-writer of history" as well as "a political chameleon that went from one extreme to another, often in rapid succession;" also, within the American party Browder became the object and "a victim of the cult of personality that led easily to personal vanity and a conviction of infallibility. He became convinced that he was independent of control from Moscow, a conviction that led ultimately to his expulsion because in reality he was not independent at all."

DAVID A. SHANNON
*University of Virginia*

JOHN P. DIGGINS. *Up from Communism: Conservative Odysseys in American Intellectual History*. New York: Harper and Row. 1975. Pp. xvii, 522. $20.00.

It is a pleasant change to read a book that addresses a historical problem rather than simply recalling a slice of past life. John P. Diggins wrote *Up From Communism* because he was puzzled by a phenomenon in recent American intellectual history. On one level it was why four people (Max Eastman, John Dos Passos, Will Herberg, and James Burnham) who before 1940 were pillars of the Old Left found themselves in the 1960s colleagues on William F. Buckley's archconservative *National Review*. This pole-to-pole intellectual migration could not have been more extreme. Eastman, to dramatize the point, changed from an associate of John Reed to an associate of John Wayne.

The larger puzzle Diggins' book confronts is why so much of the anticommunism following the Second World War was the work of ex-communists. Its hypothesis does more than point in the familiar god-that-failed scenario to Stalinism (and particularly to the Moscow purge trials of the late 1930s). Likewise, Diggins refuses to see anticommunism as entirely a response to more aggressive Soviet international policy after 1945, the so-called cold war, or to the demagoguery of Senator Joe McCarthy. Rather than these external factors, Diggins emphasizes an internal intellectual orientation of the conservatives in question. He discusses a "continuity of disdain for liberalism" that permeates the careers of his four protagonists at every stage in their career. As members of the Old Left they despised liberalism for being pragmatic, impotent, unheroic, and lacking in ideology and inspirational qualities. As conservatives in the 1960s they continued to scorn liberals for the same reasons.

The body of *Up From Communism* is a detailed, but engaging description of the life, but mostly the thought, of Eastman, Dos Passos, Herberg, and Burnham in three periods: 1912–1940, 1940–1955 (Buckley's *National Review* first appeared in that year), and 1955 to the mid-1970s. Other figures flit in and out of the account as appropriate, but Diggins' research efforts have clearly concentrated on the four central figures. Readers desiring a broader treatment might consult Diggins' other book *The American Left in the 20th Century*.

Illustrations enhance *Up from Communism* but probably also contribute to its startling price: $20. Some of the photographs are valuable documents. One shows a mixed-sex nude swimming party featuring Max Eastman and Joseph Wood Krutch around a diving board at an unidentified lake. The scene supports Diggins' argument, which the 1960s found deflating, that the Old Left was the first counterculture in twentieth-century America. In behavior as well as ideas Eastman and his colleagues dared to be different. If nude swimming was one expression of this stance, a critique of the main traditions of American liberalism was another.

RODERICK NASH
*University of California, Santa Barbara*

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GEORGE H. NASH. *The Conservative Intellectual Movement in America since 1945*. New York: Basic Books. 1976. Pp. xv, 463. $20.00.

*The Conservative Intellectual Movement in America* is an encyclopedic compendium. The dust jacket is a portrait gallery with photographs of twenty-five conservatives. The text is an attempt to describe and assess the intellectual achievements of every important conservative thinker active since 1945. The great virtue of this study is Nash's determination to do justice to all. If one's view of conservatism is a trifle dour, one might say that Nash is determined not to miss a trick. Since description plays a larger role in the book than assessment, except for the general insistence on the intellectual importance of conservatism, the reader sometimes feels rather helpless confronted with this mixed bag. Truly significant thinkers like Friedrich von Hayek and Leo Strauss are treated with roughly the same intensity and respect as journalists like William Buckley and cranks like Russell Kirk. Everyone appears on stage; nobody has a starring role. Nash's book is, therefore, a survey and introduction to the field, a series of guidelines for newcomers, but no source of new insight for the already initiated.

Consonant with his desire to be comprehensive and fair to everyone involved, Nash defines as loosely as possible: "I have designated various people as conservatives either because they called themselves conservatives or because others (who did call themselves conservatives) regarded them as part of their conservative intellectual movement." Nash seems reluctant to categorize, but he does accept the conventional distinction between the "classical liberalism" of the nineteenth century and the "traditionalists" who tend to look back to Edmund Burke and Christian doctrine rather than to John Stuart Mill and Alexis de Tocqueville. (Nash does not follow Clinton Rossiter in referring to the first group as "conservatives" and the second as "Conservatives.") To this conventional distinction, Nash adds a third category, those who rebelled against the Marxism (or New Dealism) which had formerly attracted them. Since all these people are distributed within the first two categories, one might call this an example of cross-classification, but it is certainly true that the former radicals stand out as distinctive in their adamant rejection of youthful folly. For those intrigued by Nash's brief discussions, John P. Diggins' *Up from Communism*—a study of Max Eastman, John Dos Passos, Will Herberg, and James Burnham—is an indispensable *Ergänzung*.

Nash, of course, does more than categorize and describe. The most interesting part of his work is his commentary on the inner conflicts, rivalries, polemics, and general intellectual mayhem of the conservative movement. Nonetheless, there is a reluctance to join the fray which can be praised as scholarly disinterestedness or faulted as an excess of neutrality. There is no doubt whatsoever about Nash's scrupulous research, careful documentation, and stern fairness.

ALLEN GUTTMANN
*Amherst College*

RICHARD M. FRIED. *Men against McCarthy*. (Contemporary American History Series.) New York: Columbia University Press. 1976. Pp. xii, 428. $14.95.

Few topics in the history of post-1945 America have demonstrated a more sustained capacity to stir the interest of scholars and other writers, not to mention publishers, than the activities of Senator Joseph R. McCarthy and that phenomenon of recent times known loosely as McCarthyism. The past decade has produced more than a dozen books about or relating to McCarthy and McCarthyism, ranging from the scholarly analyses of Alan D. Harper, Robert Griffith, and Athan Theoharis to the popular accounts by Fred J. Cook and Lately Thomas to the personal recollections of Roy Cohn and Senator Arthur V. Watkins. One may ponder the titles of some of those books: *Seeds of Repression*, *The Politics of Fear*, *When Even Angels Wept*, *The Nightmare Decade*. Such might be the titles of books dealing with Germany in the time of National Socialism.

A volume in the Contemporary American History Series edited by William E. Leuchtenburg, the present book rests on examination of eighty-six manuscript collections and an impressive array of government documents and other printed materials. The result is an incisive and nicely written account of the four-year effort to restrain the Wisconsin senator by an ever-changing assortment of individuals in the White House and on Capitol Hill. If in interpretation Richard M. Fried does not move far from the path charted by Griffith, he avoids the fixation on McCarthy that brought criticism from reviewers of Griffith's book.

What emerges is a tale of miscalculation and temporizing by individuals who disapproved of McCarthy—especially temporizing. At the outset of his appearance as a champion of anticommunism the Truman administration and congressional Democrats seriously underestimated McCarthy's capacity for mischief. Later, particularly after his apparent success in the election of 1950 in engineering the defeat of hostile Democratic senators, there was a tendency to overestimate his influence. As for temporizing, only a few senators were unreservedly committed to the Wisconsinite,

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but those who in varying measure disapproved of him found it difficult to get together. Republican moderates were concerned with party unity—before the autumn of 1952 in hopes that the GOP might regain national power, after that date to preserve the razor-thin (one vote) margin by which the party controlled the Senate. Democratic conservatives of the South were reluctant to do anything about McCarthy which might weaken their ability to hold the line against civil-rights legislation, and even Democratic liberals, fearing McCarthy's presumed powers of retaliation, often concluded that caution was the better part of valor.

If McCarthy benefited from the contingencies of partisan politics, according to Fried, he also benefited from the exigencies of the cold war, for even his most seething opponents in Congress shared his conviction that Soviet communism menaced the United States. In such circumstances it was difficult to press the contention that a rigid observance of the Bill of Rights was as important as action to guarantee the security of the republic. But alas, the Republicans returned to power in 1953, and before long McCarthy was at odds with the new administration. Meanwhile the cold war underwent a thaw of sorts. The changing configurations of domestic politics and the cold war, Fried explains, prepared the way for the censure and eventual demise of the Wisconsin senator.

Fried's book may not appeal to more impassioned students of the McCarthy saga, for it is devoid of literary pyrotechnics. Writing with remarkable detachment, the author makes little effort to identify heroes or villains, and indeed is surprisingly restrained in his treatment of McCarthy. Not that he harbors the slightest sympathy for the senator. He simply is trying to follow Ranke's dictum that the historian should write history as it happened.

JOHN EDWARD WILTZ
*Indiana University, Bloomington*

PHILIP TAFT. *Rights of Union Members and the Government*. (Contributions in American History, number 39.) Westport, Conn.: Greenwood Press. 1975. Pp. xv, 348. $14.95.

Philip Taft has written a thorough book about the Labor-Management Reporting and Disclosure Act of 1959 (LMRDA), an important federal law designed to protect the rights of union members and curb racketeering and corruption by union officials. Though most union leaders are honest and some are idealists who have devoted their lives to helping fellow workers, a minority have been corrupt and dictatorial. These have tarnished the reputation of the labor movement.

Labor has tried to clean its own house by passing codes of ethics and disciplining some malefactors. But the federal government has been the prime mover in the campaign against wrongdoing. Between 1957 and 1959 a committee headed by Senator John L. McClellan investigated and revealed extensive crooked dealings in the Teamsters' Union as well as abuses in other unions. The resulting act provided a "bill of rights for union members" guaranteeing to them the rights of free speech, discussion of union business, participation in union elections, and holding union office. Complaints may be directed to the Secretary of Labor. The law also defines the financial responsibilities of union officers. The 1959 LMRDA provides for the most comprehensive regulation of internal union affairs ever enacted in the United States. Enemies of organized labor hoped to use it to undermine labor unions, but unions generally have not experienced a reduction in bargaining power.

The battle for honest and democratic unions is a fascinating story. This volume deals with the subject in a detailed, analytical, and unexciting way which makes for dull reading. The book is valuable, however, to those who need a thorough and objective reference work, and Taft has made an original and substantial contribution to labor history.

JONATHAN GROSSMAN
*U.S. Department of Labor*

WALTER JOHNSON, editor. *The Papers of Adlai Stevenson*. Volume 5, *Visit to Asia, the Middle East, and Europe, March-August 1953*. Boston: Little, Brown and Company. 1975. Pp. xvi, 512. $17.50.

When a defeated presidential candidate is articulate, enquiring, internationally-minded, and free of family ties, the obvious solution to the problem of what to do in the wake of defeat is to embark on a world tour and to be his own Boswell en route. In this way he can gracefully detach himself from a political environment necessarily dominated by the victor and still retain a claim on the public's eye and ear. Small wonder then that Adlai Stevenson so readily embraced the offer of the editor of *Look* magazine to visit Asia, the Middle East, and Europe in the spring and summer of 1953 and report his impressions for its readers.

Yet as a later reader follows the intimate narrative of these travels, recorded in the fifth volume of *The Papers of Adlai S. Stevenson*, he may well wonder whether anyone who does not command the endurance of an ox and the pen of a recording angel could be well advised to take on such a task. The record assembled here, in meticulous detail, of airport arrivals and departures, of embassy briefings, of pompous adlocutions from visited digni-

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taries, of inspections of guards of honor and Kibbutzim, of sightseeing of monuments and beauty-spots in the company of reporters and rabble, of questionings by press and public, of "few, well-chosen words" at appropriate moments, of courtesy calls and obligatory banquets—all this going on day after day in every sort of climate and in the relentless publicity attaching to a world figure—all this might well make the hurly-burly of the hustings seem an ordered, relatively manageable ritual by comparison. Yet, despite natural groans and grumbles, Stevenson seems to have thrived on it. The Illinois Hamlet is much less in evidence here than the indefatigable Phineas Fogg.

It is hard to assess what value this carefully preserved, backstage record of this globe-girdling will have for historians. It tells little, of course, about politics in America. Its account of world affairs, even as they appear at the moment and point of the traveler's impact on them, is necessarily superficial. Yet no full understanding of Stevenson and his place in the politics of America and the world would be possible without it. Often in relation to particular personalities or crisis areas—Chiang Kai-shek, Saigon, Nehru, to cite a few examples—it affords insights of direct and immediate historical relevance. Often, to be truthful, it does not. But over and above this it affords, to anyone who follows this extraordinary odyssey in the detail with which it is here recorded, a vivid cumulative impression of how the cosmic chaos of 1953 appeared to a humane and informed American citizen of the world.

H. G. NICHOLAS
*Oxford University*

NUMAN V. BARTLEY and HUGH D. GRAHAM. *Southern Politics and the Second Reconstruction*. Baltimore: Johns Hopkins University Press. 1975. Pp. xvi, 233. $10.00.

Why have not urbanization, industrialization, and the removal of the institutions which V. O. Key saw inhibiting an organized system of electoral competition—disfranchisement, malapportionment, one-partyism, and Jim Crow—produced a New (Dealish) South? Numan V. Bartley and Hugh D. Graham approach this question through short, sprightly, and incisive narratives of every significant statewide election campaign in the South from 1945 to 1972, as well as analyses of election statistics that fill 36 tables and 46 figures of this 200-page work. They conclude that though a politics of economic self-interest would produce a biracial lower-class ("populist") coalition against the more affluent whites, this class division of the electorate has more often given way in recent Southern politics to an upper-class white/black alignment against rural and lower-class white racists, or, even more darkly, to a "Bourbon" pattern in which blacks had no white allies. (Such "New South moderates" as Jimmy Carter do not fit their binary scheme very neatly.)

This is excellent political journalism. Knowledgeable and commonsensical, the authors offer a thorough, but nonetheless lively, analysis of events. Yet if Barley and Graham display the virtues of newsmen, they do not escape some of their faults: the book lacks a foundation in political theory; it skirts difficult questions concerning the connection of society and politics; and its statistical analysis is unsophisticated—a fact disarmingly admitted by the authors in their appendix. Ignoring recent political science literature on electoral competition, political beliefs, and rational decision-making, the authors paint confusing pictures of party strategies and "rational" or "realistic" choices by voters. Nor do they relate changes in Southern social structure to alterations in politics. Does the category "lower class," for instance, mean the same thing in the 1970s as it did in 1949? As for their methodology, by collapsing 1,134 counties into 24 ecological areas, they have vastly decreased the reliability of their statistical estimates. Their unexplained regional and sociological groupings obscure the determinants of political outcomes. By incautiously generalizing in the text (despite qualifications in the appendix) from data drawn only from homogeneous areas, they may have misled us about the class basis of certain politicians' appeals. Finally, by using total votes, rather than eligible voters, in the denominators of many of their equations, they leave us hopelessly confused about changes in the candidates' voting bases from primary to general elections and about the probably crucial effects of migration and increased electoral participation on political outcomes.

J. MORGAN KOUSSER
*California Institute of Technology*

EARL BLACK. *Southern Governors and Civil Rights: Racial Segregation as a Campaign Issue in the Second Reconstruction*. Cambridge: Harvard University Press, 1976. Pp. xiv, 408. $16.50.

This exhaustively researched, carefully analyzed, well-organized, and clearly written study of racial segregation as an issue in southern gubernatorial campaigns from 1950 to 1973 is a significant addition to the growing literature on recent southern politics. Among its conclusions are: that racial segregation was not a prime campaign issue before 1954 (mainly because the candidates did not make it an overt issue and because their constituents assumed that it was a settled question); that racial segregation became a major issue in the decade following the *Brown* decision during which time

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strong segregationist candidates generally won; and that racial segregation gradually declined as a campaign issue after 1965 during which time moderate segregationist and nonsegregationist candidates began to win more contests. This change occurred sooner and more often in the six peripheral states than in the Deep South. The author rightly believes that federal intervention through statutes and Supreme Court decisions had a direct and positive effect upon southern race relations and an indirect but salutary effect upon political campaigners' stances. The dramatic increase of black registration and voting, despite a concomitant rise in white registrations, provided a partial check on stem-winding race baiting. The growing strength of the Republican party in the South, moving the region away from the stifling one-party tradition, also curtailed racist oratory. Strong segregationists were usually staunch economic conservatives, while nonsegregationists held more liberal economic views.

These conclusions are neither startling nor new, but this model study in the tradition of V. O. Key's monumental work reinforces previous assumptions based upon less-hard data. In view of declining racism and increased economic liberalism, Earl Black views the future of southern political campaigns (and thus of southern politics) optimistically. I am inclined to share his view, even though some competent students of the subject draw more pessimistic conclusions.

MONROE BILLINGTON
*New Mexico State University*

JAMES MACGREGOR BURNS. *Edward Kennedy and the Camelot Legacy*. New York: W. W. Norton and Company. 1976. Pp. 383. $11.95.

In 1960 James MacGregor Burns received plaudits for his sensitive and informative *John Kennedy: A Political Profile*. It served, whether by design or accident, as a valuable campaign biography for JFK, and it remains the best prepresidential account of his life. Burns clearly hopes that lightning will strike twice and that his *Edward Kennedy and the Camelot Legacy* will be as valuable for Edward Kennedy as the earlier book was for John. His study, Burns emphasizes, is not a commissioned or official biography, although he did obtain exclusive materials from Kennedy and did check various facts with the senator and his staff. The book is enlivened by a large number of superb political cartoons and a fine selection of photographs.

In assessing his subject Burns strives to be objective, but his bias in favor of Kennedy is obvious. The darker sides of Kennedy's career are certainly not ignored, but they are handled gently. His suspension from Harvard for cheating is accepted as a youthful mistake from which he learned a valuable lesson. His fondness for alcohol and women are mentioned several times, but the sting is removed by allusions to the fact that others in Washington do the same thing; Kennedy's main fault seems to be that he is not very discreet. As for the tragic incident at Chappaquiddick, Burns' account is relatively brief, offers nothing new, and basically accepts Kennedy's explanation without serious challenge.

Burns predicts that Ted Kennedy would be an innovative, activist, and, above all, compassionate president committed to social and economic justice. He would carry on the legacy of his dead brothers and fight for their goals. Burns is particularly impressed by Kennedy's thorough command of the issues, by his experience and record in the Senate, and by his unique ability to appeal to Americans. He is, Burns, argues, in many respects "far better prepared, far better equipped, than John Kennedy had been to lead the nation."

Burns claims that his study is not a campaign biography, but rather "a biography timed by the author to appear in a year, 1976, one of several (1980, 1984) in which Edward Kennedy might be nominated for President." The wording is ingenious, but few readers are likely to regard it as much of anything but a campaign biography. And particularly if Ted Kennedy never reaches the White House, later historians will probably wish that Burns had spent more time on Kennedy's actual career and less on speculation (a third of the book) about what kind of a president he might be.

JIM F. HEATH
*Portland State University*

## CANADA

LEWIS H. THOMAS. *The Renaissance of Canadian History: A Biography of A. L. Burt*. Buffalo: University of Toronto Press. 1975. Pp. xiv, 189. $15.00.

WELF H. HEICK, editor. *History and Myth: Arthur Lower and the Making of Canadian Nationalism*. Vancouver: University of British Columbia Press. 1975. Pp. xxii, 339, $16.95.

Both Alfred Leroy Burt and Arthur Lower were born and raised in southern Ontario on the edge of the vast Precambrian Shield, a love for which has drawn many a historian into an exploration of the Canadian past. Their professional training was superb: the University of Toronto followed by Harvard (Lower) and Oxford (Burt). Upon returning to Canada both Lower and Burt took up academic appointments in the West, Lower in Manitoba and Burt in Alberta. While at Oxford as a Rhodes scholar, Burt shared the Beit Prize in

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colonial history with Lewis Namier. While at Harvard, Lower was told that Canada was only a cultural hinterland of the United States. "Secretly I hoped," Lower later wrote, "that it could become something more and that I could aid in making it more." It is tempting to see in these early experiences the outlines of each man's professional career.

Burt became the scholar nonpareil. *The Old Province of Quebec* (1933) and *The United States, Great Britain and British North America* (1940) are two of the chief glories of what Lewis Thomas, in his incisive and sympathetic biography of A. L. Burt, has aptly called the renaissance of Canadian history. Anyone who has ever been exposed to Burt's scholarship—as I was under the direction of one of Burt's first doctoral students—must marvel at the achievement of that remarkable generation of historians who traveled to the Public Archives at Ottawa at their own expense, lived in cheap boarding houses, copied out documents by hand, and in the heat of the Ontario summer laid down the foundations for the scientific study of the Canadian past.

Lower's career developed along different lines. It is difficult to image Burt's writing, as Lower does in the preface to Welf Heick's collection of his essays, that perhaps he is not a historian at all. For the central fact of Lower's view of the past is its relevance to contemporary concerns. Hence Lower could write of himself as a publicist, a journalist, a propagandist, "a child of the here and now." The vast majority of the contributions listed in Lower's bibliography fall into the category of the public essay. It is from these materials that Heick has selected the essays reprinted in *History and Myth*. Seven essays on history and historiography are followed by twenty-four papers on values, biculturalism, civil liberties, immigration, education, external relations, and nationalism.

For both Lower and Burt nationalism was the key issue in Canadian history. Both historians arrived at almost identical conclusions. They were aware of the tremendous power of British imperialism on the one hand and American continentalism on the other. To withstand these forces, Canada needed to develop a sense of national identity. That identity would, however, have to be based on a full and frank acceptance of cultural and linquistic diversity. Thus, as early as 1926, Burt wrote: "a . . . recognition of our dual nationality must be at the very foundation of the Canadian mentality. . . ." Likewise, Lower was to point out in his seminal essay "Two Ways of Life: The Primary Antithesis of Canadian History" (1943) that "the fate of the two peoples seems indissolubly linked."

In these two volumes L. H. Thomas and Welf H. Heick have made substantial contributions to our knowledge of the Canadian intellectual tradition.

LAURENCE S. FALLIS
*Las Cruces, New Mexico*

ALAN F. J. ARTIBISE. *Winnipeg: A Social History of Urban Growth, 1874–1914*. Montreal: McGill-Queen's University Press. 1975. Pp. xiv, 382. $18.00.

The first piece of writing about Winnipeg, G. B. Elliot's *Winnipeg As It Is*, appeared in 1874, a few months after the incorporation of the city. Most of the subsequent books about the city have been souvenir volumes, historical sketches, and personal reminiscences without much more than a nod to social, economic, and political forces.

Alan Artibise has undertaken a deeper investigation into the history of Winnipeg, which stands at the confluence of the Red and Assiniboine Rivers in Manitoba, a site where there were trading posts of the Northwest Company and the Hudson's Bay Company from the eighteenth century. When the Hudson's Bay Territory was taken into the Dominion of Canada in 1870 the strategic location, then known as Red River or Fort Garry, began a period of rapid growth, directed in the main by those who were soon to become the commercial and financial leaders of the city. In 1871 there were fewer than seven hundred persons in what was to become the city of Winnipeg, and the population grew to over 200,000 forty years later. Artibise's book, which examines the years 1874–1914, spans the period of Winnipeg's efforts to provide roads, water, sewers, health care, transportation, electrical energy, a school system, and a university against the background of the national railroad-building era and the opening of the West to an increasing flow of immigrants, many of whom came from quite different backgrounds from those of Winnipeg's earliest Scottish, French, or later Ontario-born leaders.

The period under discussion was one in which most North Americans accepted the thesis that what every city, town, state, or province needed was expansion. The growth syndrome, which is still evident in Toronto, Montreal, Calgary, and Vancouver, was probably strongest in the pre-World War I era. Capitalism was accepted, capitalists—called capitalists—were revered. It is not surprising then that Winnipegers accepted the leadership of the Ashdowns, Logans, Andrews, and Macdonalds. Before making judgments about control by a commercial elite one should perhaps have a look at those they defeated at the polls. Were the winners politically astute or were there no rivals? Certainly the a lack of "clash of values" in a city government run by commercial interests

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is very evident and accounts in part for the direction which Winnipeg took.

The book provides an excellent analysis of the influence of the railways on spatial growth and social development, and the author's examination of the concept of water transport and its involvement with civic politics is almost classic. there is, however, a tendency to confuse ethnic segregation and class structure. For example, the conditions which resulted in the 1919 Winnipeg general strike may have had some ethnic overtones, but they were rooted in economic and class conflict, although it must be admitted that those who were concerned with breaking the strike made use of racist tactics.

Alan Artibise has written a book that needed to be written and that should be joined by urban histories of other Canadian cities.

WILLIAM A. MCKAY
*Pickering, Ontario*

RICHARD JONES. *L'idéologie de l'Action Catholique, 1917–1939.* (Histoire et sociologie de la culture, number 9.) Quebec: Les Presses de l'Université Laval; distributed by International Scholarly Book Services, Portland, Ore. 1974. Pp. 359. $12.00.

It is difficult to believe that just over thirty years ago there existed, on the border of New York state, a society which was unashamedly profascist and violently anti-Semitic. Nevertheless, this study of what was Quebec City's largest daily newspaper by the late 1930s supports that conclusion. *L'Action Catholique* was not only North America's second largest French newspaper (after *La Presse* of Montreal), it was subsidized by the archbishop's office and so unofficially reflected the Church hierarchy's views of the events of the day in "the priest-ridden province." Its contributors were priests and laymen, including several doctors. (The province's educational system, however, did not produce francophone economists, sociologists, or professional journalists.)

Almost two-thirds of this book chronicles the newspaper's revealing attitude toward interwar Europe, where it perceived an apocalyptic struggle between the forces of light and the forces of darkness: The Roman Catholic Church, defended by good dictators (Salazar, Dollfuss) and troublesome ones (Mussolini, Hitler, Franco), faced the judeo-masonic-bolshevik conspiracy. The WASP powers, including the Jew-infested United States, were frequently duped by the latter.

*L'Action Catholique's* chief source for analyses of European affairs was the French Catholic daily *La Croix* (which an embarrassed Pius XI was finally forced to take in hand). The *La Croix* pieces were the most informed; the contributions from the Quebec side cited by Richard Jones are of an appalling and monotonous stupidity and supersition (the more colorful explained that Lenin was really a Prussian Jew named Zederblum or described how the *franc-maçons* carried out the French Revolution, etc.). Fresh explanations of events were so rare that Jones was reduced to counting particular clichés (for example, "Mussolini was fighting the Masons and was, in turn, being attacked by them: 234 lines, 11 references").

The newspaper's treatment of Canadian problems, as recorded here, was hardly enlightened either. Jewish immigrants and Jehovah's Witnesses were special targets. The Communists were blamed for the one-piece bathing suit (1930s version). Compulsory primary education was attacked, the fundamentalist side in the Scopes trial defended.

Jones has strung the quotations together with objectivity and detachment. But he describes his subjects' opposition to immigration to the province during the Depression on the grounds of extant unemployment as a mere pretext, and their effort to defend their "closed society" against the dominant North American values as a disguised attempt to protect their privileged status within Quebec. This explanation absolves both international Catholicism and the enemies of Quebec culture of all responsibility for the paranoia reflected here. We are left wondering who read the newspaper and to what extent its general "disquiet, frustration, incertitude and fear" characterized Quebec society as a whole in this period.

JOHN HELLMAN
*McGill University*

ALOYSIUS BALAWYDER. *Canadian-Soviet Relations between the World Wars.* Toronto: University of Toronto Press. 1972. Pp. ix, 248. $12.50.

During the century prior to the Statute of Westminister in 1931 Canada's story was largely one of progress from colony to nation. By the 1920s Canada was taking over control of its own foreign relations from Great Britain. For several years, however, many Canadians believed that Canada's foreign policy should be worked out within the traditional framework. That is why the main emphasis in foreign relations was on trade, and why Canadians tended to follow British leadership in foreign politics. There were, however, other factors which helped determine Canada's attitude toward her northern neighbor, Russia. These were the pressure of ethnic groups, the activities of the Comintern in Canadian domestic politics, and the proximity of the United States. In the interwar period this last was the least significant.

Aloysius Balawyder examines all of them. He

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shows how British example led to Canada's resumption of trade relations with Soviet Russia in 1922 and to *de jure* recognition in 1924; how British influence and the activities of the Comintern brought about the rupture between Canada and Russia in 1927 and then the resumption of relations in 1929 despite the opposition of the United States. He further shows that the Soviet practice of dumping goods aroused the opposition of certain Canadian producers and resulted in an undeclared economic war between Canada and Russia in 1931, and explains why, after three years, Canadian attitudes changed under the pressure of other economic groups and the influence of the League of Nations, leading to a removal of the embargo in 1936. Most of Canada's decisions were obviously made without any thought to a consistent, long-range policy toward Russia.

This book is neither a spy story nor a political tract in which the Russians appear as bloody Bolsheviks or the Canadians as bloody-minded fascists. The author has done a thorough piece of work based on Canadian and British sources and Russian printed documents. His pages are packed with solid facts which make for slow reading but are of great value to the student. Canadians have long needed a scholarly study of this nature. Personally I would like to see a sequel from Balawyder's pen covering the Second World War and the subsequent thirty years.

GEORGE F. G. STANLEY
*Mount Allison University*

ROBERT H. BABCOCK. *Gompers in Canada: A Study in American Continentalism before the First World War.* Buffalo, N.Y.: University of Toronto Press. 1974. Pp. x, 292. $4.95.

"Our aim should be to unite the workers of our continent," Samuel Gompers told the AFL convention in 1897. Robert Babcock's *Gompers in Canada* is an examination of Gompers' effort to bring the labor movement in Canada under his control before the Great War. The decisive episode in the story is the account of the Berlin (Kitchner) Convention of the Trades and Labor Congress of Canada in 1902. There the delegates of the AFL affiliates in Canada used their numerical strength to force the expulsion of nearly a fifth of the TLC membership which was associated with other labor organizations (primarily the Knights of Labor), to impose the AFL attitude toward the role of government in labor disputes upon the TLC, and to elect the AFL organizer in Canada, John Flett of Hamilton, to his first term as president of the TLC. In succeeding years repeated efforts by some TLC officials, most notably TLC secretary P. M. Draper, to establish a larger and more autonomous role for the Canadian organization in the Canadian labor movement and in labor politics were thwarted by the influence of the AFL affiliates who accepted Gompers' definition of the TLC as equivalent to a state federation of the AFL. The result of the Berlin Convention, Babcock concludes, was that "the Trades and Labor Congress of Canada lost most of its influence over the structure and organization of Canadian labour, and became little more than a lobby charged with the defence of the international trade-union movement in the corridors of the Canadian parliament."

But, as Babcock demonstrates, Gompers did not achieve his goal of uniting "the workers of our continent." Rather, Gompers' insensitivity to the regional and cultural disparities of Canada and his heavy-handed use of the TLC to combat any labor initiative in Canada that smacked of dual-unionism had the effect of encouraging radical unionism in British Columbia and confessional unionism in Quebec and perpetuating the Provincial Workmen's Association in Nova Scotia. By 1914 the international unions had done much to improve the working conditions of their Canadian members, and the TLC was the dominant force in the Canadian labor movement. But the movement itself was deeply divided. "The A.F.L. operated as a divisive force when the Trades and Labor Congress was transformed from a body unifying Canadian unionists into an arm of the international crafts," Babcock writes. "In a country wracked if not wrecked by regionalism, the loss of a truly national labour institution was doubtless unfortunate."

*Gompers in Canada* is the first thorough study of the development of international unionism in Canada before the First World War. This book, which was awarded the 1976 CHA-AHA Corey Prize, gives historians of labor, of Canada, and of Canadian-American relations an excellent survey of the early manifestations of labor imperialism in Canada.

ROBERT CRAIG BROWN
*University of Toronto*

## LATIN AMERICA

SHERBURNE F. COOK and WOODROW BORAH. *Essays in Population History: Mexico and the Caribbean.* Volume 2. Berkeley and Los Angeles: University of California Press. 1974. Pp. xv, 472. $16.50.

NICOLÁS SÁNCHEZ-ALBORNOZ. *The Population of Latin America: A History.* Translated by W. A. R. RICHARDSON. Berkeley and Los Angeles: University of California Press. 1974. Pp. xv, 299. $17.50.

That a full and detailed general synthesis on Latin American population history can now be written is

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a sign of the advances which have been made in the last two decades. From a state of almost total neglect Latin American demographic studies have made such rapid strides that they are approaching the sophistication of similar work on Europe and the United States. Even in the last six or seven years the advances have been remarkable. Students should compare the present volume with Nicolás Sánchez-Albornoz's earlier text (written in collaboration with José Luis Moreno), *La población de América Latina: Bosquejo histórico* (1968).

The author, an Argentine historian now teaching in the United States, has words of caution. He finds large gaps in the chronological record, and warns that, "A complete adjustment has not yet been achieved between demographers and historians" (p. 3). But his command of what is now a large bibliography, and the admirable job he does of presenting the major debates in the field of Latin American population studies, show that there are also grounds for optimism—especially when the field is in the hands of a writer of such sophistication.

Sánchez-Albornoz covers a period which runs from pre-Columbian times to the year 2000. He divides these many centuries into five eras: hunting peoples, native agrarian civilizations, the conquest, the colonial and neocolonial era, and the modern population explosion. In each of these subdivisions he presents the major materials used by demographers and historians, and judiciously outlines the important issues of fact and interpretation.

With regard to the centuries just before the arrival of the Iberians the major debate now emerging is over long-term trends. Does the history of pre-Columbian populations show a lineal growth, or were there a series of Malthusian crises, with well-worn phases "A" and "B," similar to the pattern in Europe before the Industrial Revolution? Sánchez-Albornoz does not decide, but seems to lean toward Malthus.

The debate over the conquest period, which the author defines as 1492–circa 1660, is still raging. Did a massive Amerindian population collapse occur after the arrival of Europeans? The "Berkeley School," via its house organ, the Ibero-Americana series, says yes. (See also below.) Ángel Rosenblat *et al.* (plus a large contingent of Spanish scholars) say no. For obvious reasons the waters swirling around this debate have become thoroughly clouded, if not muddied, by political and nationalistic detritus. Proponents of the "black" and "white" legends, students of comparative imperialism, and simple propagandists have been unable to resist a splash. Sánchez-Albornoz decides that the population collapse is now beyond question and attempts to raise the debate to a higher plane. What was the aboriginal population before the decline began? What caused the disaster? Was it homicide, loss of will to survive, massive economic and social dislocations, or epidemic disease plus lack of physiological immunities? The author feels the last two are of prime importance and deserve further study.

He then goes on to study the consequences of the demographic collapse. Much of his discussion covers familiar ground, and he concludes, as the Berkeley School has done, that, "The numerical decline of the Indian population permitted the complete domination of the conquered people by a minority without the necessity of any intermediaries. In the long run the Spaniards did not need to make use of native institutions or the native nobility, contrary to what happened in Portuguese or British India, for example" (p. 65).

The fourth period considered runs from c. 1660 to the 1930s, two centuries of colonialism and one of independence. This phase was one of slow growth and recovery, ending with the immigrant waves from Africa and Europe and the new forces, such as growth in productive and export capacity, which set the stage for the modern population explosion.

Sánchez-Albornoz ends this excellent study by analyzing this explosion and projecting its effects toward the year 2000. He finds a whole continuum of growth and its effects, varying according to nation. In some countries, such as Uruguay and Argentina, the explosion has peaked and passed, although rapid urbanization continues. In others, such as Venezuela and Costa Rica, the peak has just passed, but the rush to the cities has just begun. Mexico, Panama, and Honduras are now growing at staggering rates, while in nations such as Bolivia, Haiti, and El Salvador growth rates are still increasing and the rush to the cities has hardly begun. He concludes that, "Although the peak rate has not yet been reached in some countries, on the whole the majority seem to have passed it . . ." (p. 252).

Volume two of *Essays in Population History* by Woodrow Borah and the late Sherburne F. Cook is a handy illustration of the new sophistication which has encouraged a survey such as the one discussed above. In volume one the authors expanded their well-known work on colonial Mexico to new areas such as Colombia and the islands of the Caribbean. Now they return to Mexico, and while the book begins with a long population history of Yucatan, an area new to the authors' writings, their main theme is a more detailed demographic analysis of the Mexican situation within a larger time frame. Chapters covering such diverse themes as racial groups, age at marriage (1690–1960), and age at death and cause of death

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since 1860 illustrate the breadth of coverage and the length of time covered.

It is impossible to describe the richness of the materials in this volume in the space allotted here, but two points are in order. One misses the grace and elegance found in the earlier writings, both joint and individual, of the two authors. Does a total immersion in the tepid seas of quantification drown style? Not completely, it is apparent, but there are dangers. Above all one must continue to applaud daring and imagination. The authors go courageously toward the results of their calculations and deductions, however startling or bizarre they at first appear. If a forecast is in order, it is that the early howls of indignation which have greeted so many works by these authors will soon yield to a higher level of debate, and to deeper knowledge of many of the above issues.

MURDO J. MACLEOD
*University of Pittsburgh*

RICHARD S. MACNEISH and MELVIN L. FOWLER *et al.* *The Prehistory of the Tehuacán Valley.* Volume 5, *Excavations and Reconnaissance.* Austin: University of Texas Press for the Robert S. Peabody Foundation. 1975. Pp. xii, 529. $29.50.

Based on fieldwork carried out in the Tehuacán Valley of southcentral Mexico during 1960–1965, this monograph series ranks as one of the most complete publications of regional prehistoric research ever undertaken. Earlier volumes presented many of the raw data—combined with relevant ethnographic, historical, and technical detail—while this penultimate volume attempts to place it in meaningful context, from individual components of single archeological sites to the broader geographical overview during a time period ranging from about 10,000 B.C. to the Spanish Conquest in 1521. The final volume will provide a vehicle for developing hypotheses about the major changes in cultural configurations which are clearly set out in this contribution.

Following a brief introductory section, chapters 2-6 outline the various sites excavated in five contrasting environmental settings within the valley, including description of the excavation techniques employed. Chapter 7 describes additional sites and localities surveyed either in the course of initial reconnaissance required to identify sites most worthy of excavation or as part of a deliberate effort to supplement information on settlement pattern and differential use of distinct microenvironments by actual excavation of key sites. The last chapter comprises a summary of the entire culture-chronological sequence and offers some comments on its meaning which are intended to provide a prelude to the last, and more explanatory, work in the series.

The Tehuacán research was characterized throughout by close attention to ecological considerations, a strategy made successful both by careful planning and by the excellent preservative qualities of the semiarid environment. Employing extensive plant remains, animal bones, and human coprolites, systematic ethnobotanical and ethnozoological analysis was conducted by Kent Flannery, Eric Callen, Paul Mangelsdorf, and other specialists to give an unusually good picture of subsistence and diet. From this, in relation to associated cultural materials (including wooden artifacts, cordage, textiles, and basketry as well as stone tools, pottery, burials, and structures) has come our clearest information on the change from hunting and gathering to food production in the entire New World. Computer plotting of individual occupation or activity areas within excavated sites represents a novel approach that fulfills the stated aim of stressing functional interpretations and differentiation of seasonal and environmental deployment of subsistence and other extractive activities.

In short, one finishes the book with an appreciation of the entire panorama of cultural change from simple, nomadic food-collecting bands toward town life that incorporated rich religious and ceremonial traditions, extensive control of water supply, marked population growth, developed craft specialization, formalized trade, and effective political organization. The final task before the authors is to sort out the systemic relationships between all of these in order to reach a better understanding of the shift away from a nonagricultural way of life toward nascent civilization. In many respects this will be the supreme test in validating the enormous amount of labor which has gone into the project. At the same time, the meticulous presentation of published data, concluded in this volume, will furnish future investigators with ample material from which to contrive alternate explanations which are more to their own liking.

CREIGHTON GABEL
*Boston University*

EBERHARD STRAUB. *Das Bellum Iustum des Hernán Cortés in Mexico.* (Beihefte zum Archiv für Kulturgeschichte, number 11.) Cologne: Böhlau Verlag. 1976. Pp. 222. DM 46.

The title of Eberhard Straub's treatise really covers the first three chapters of the book. In those chapters he first discusses what causes for waging war the western European world regarded as just before the Vitoria lectures. He then traces through

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the letters of Hernán Cortés on the conquest of Mexico the assertion of each of these causes. The last three chapters of the book, and in many ways the more provocative ones, deal with the artistry in the letters. Straub finds that Cortés, a man of far wider education than is normally assumed, aimed at a work of justificatory propaganda that would appeal to the mind of his age, establishing both the justice of his actions and his fame as a great Christian hero. Chapter 4 compares Cortés's style of writing, thought, and argument with that of Caesar in the *De Bello Gallico*, demonstrating how much the latter served as model. Chapter 5 adds the further paradigm of the romances of chivalry, showing how completely Cortés strove to prove that he possessed the heroic qualities and behavior demanded by such widely read and deeply beloved masterpieces. In demonstrating the writing of the letters as propaganda and literature, Straub raises questions about their value as a source of history which are not easily resolved; he would concede that they are history in the sense of literature. The final and most controversial chapter is a thoroughgoing attack upon the credibility of Bernal Díaz del Castillo, who, according to Straub, merely filled out the material of the letters of Cortés and the history of López de Gómara in a tendentious attempt to prove his own right to fame.

WOODROW BORAH
*University of California, Berkeley*

CHARLES H. HARRIS III. *A Mexican Family Empire: The Latifundio of the Sánchez Navarros, 1765–1867*. Austin: University of Texas Press. 1975. Pp. xvii, 410. $15.00.

Of the various institutions that have shaped the history of Mexico and structured the lives of its people, the *latifundio* is probably the most significant. These great landed estates were the dominant socioeconomic and political feature until their dissolution commenced with the success of the Revolution of 1910. At its most extensive stage the *latifundio* of the Sánchez Navarro family of Coahuila was composed of seventeen haciendas and covered an area of 16.5 million acres within the present borders of Coahuila, Durango, Zacatecas, and Nuevo León.

In six lengthy chapters (the family and the land; ranching; labor; *latifundio* production; commerce; and politics) Harris provides an expansive view of the development of the *latifundio* as a socioeconomic institution and of the rise and fall of the family itself. Viewing the Sánchez Navarros within the context of the hacienda system, Charles H. Harris establishes that the family conforms to some of the historical stereotypes concerning *hacendados* and haciendas: concentration of landownership, debt peonage, and inefficient use of land coupled with reliance on traditional methods of production. But he cautions that in each case regional variations in the institution regarding size, the extent of patronage, and the use of land for commercial production should be researched further. Perhaps eventually there will be a strengthened case for considering the northern hacienda as a distinct type. There are other stereotypes to which the Sánchez Navarros do not conform. The family amassed property through legal transaction, not usurpation; they built the *latifundio* primarily to make money, not for prestige purposes; their objective was financial liquidity (bolstered by extensive ranching and commercial activities) and not just self-sufficiency for their estates.

Harris concludes that the development of the *latifundio* was only one element in the rise and success of the Sánchez Navarro family. Unremitting labor, shrewdness, and luck were responsible for its rise; political pragmatism, kinship politics, and covert machinations for its success. A political gamble (the family's fateful alliance with Emperor Maximilian in the 1860s), not indolence or business mismanagement, caused its collapse.

Harris' research is thorough and despite the infinite complexity of the topic he has combined narrative, analysis, comparison, and hypothesis in an enviable manner. It is superb history, illuminating the intricate links between the activities of the family and the evolution of the great estate itself. This book is testimony to Harris' commitment to painstaking but rewarding scholarship.

ANTHONY T. BRYAN
*University of Rhode Island*

JAIME E. RODRÍGUEZ O. *The Emergence of Spanish America: Vicente Rocafuerte and Spanish Americanism, 1808–1832*. Berkeley and Los Angeles: University of California Press. 1975. conditions land $17.00.

This book treats what Jaime Rodríguez calls "Spanish Americanism," an effort by a cosmopolitan group of liberal intellectuals, politicians, and diplomats between 1808 and 1830 to promote Hispanic unity. The author identifies Vicente Rocafuerte as a representative example of this group and devotes a large part of the book to a detailed narrative of his career from 1820 to 1832, based on impressive archival material from several countries. Born in Guayaquil, Rocafuerte was wealthy and well educated and served as a delegate to the Spanish Cortes in 1814. He became a conspirator for Cuban independence and a leading diplomat for his adopted country Mexico throughout the 1820s. He was also active in Mexican politics, first

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as a promoter of republicanism in 1822–23 and later as a liberal opponent of Bustamante's regime in 1831–32. He returned to Ecuador in 1833, where he served as president.

The Spanish Americanists, argues Rodríguez, were deeply influenced by the Spanish liberal tradition. As long as the hope for a liberal Spain remained, they envisioned a Hispanic commonwealth centered in the peninsula. When Spain became absolutist once again in 1823, they promoted Spanish-American unity and worked together to secure European recognition and commercial treaties. Disillusionment set in about 1830, and cosmopolitan optimism gave way to exclusive national concerns.

The theme of Spanish Americanism is well conceived, important, and heretofore unstudied. Rodríguez has made a considerable contribution in bringing it to our attention. Unfortunately the theme becomes lost in the middle chapters amid the details of Rocafuerte's career. The author does not pursue important definitional and conceptual matters, which are often relegated to suggestive footnotes. One such matter is the relationship between Spanish Americanism and the Western Hemisphere idea—the ambivalence toward identification with the United States shown by Rocafuerte and his fellow adherents to Spanish liberal constitutionalism. In this regard, discussion of Bolívar's ideas and of the Panama Congress would have been revealing. Another matter involves political terminology, which Rodríguez tends to use loosely. He never really defines the aspects of Spanish liberalism to which Rocafuerte adhered. For example, was he anticlerical before 1831? He had close ties in the 1820s with future political conservatives Lucas Alamán and Francisco Fagoaga, and he was a centralist before turning to federalism. Indeed, Rocafuerte's Mexican career (like that of José María Luis Mora) gives further testimony to the lack of an ideological conflict between liberalism and conservatism before 1830 and to the sway of a benign and nonideological constitutional orientation which was at the heart of Spanish Americanism.

CHARLES A. HALE
*University of Iowa*

MICHAEL P. COSTELOE. *La Primera República Federal de México (1824–1835): Un estudio de los partidos políticos en el México independiente*. Mexico City: Fondo de cultura Económica. 1975. Pp. 492.

Few periods of Mexican history confound historians quite as much as the three decades that followed independence in the early 1820s. During those years Mexico experienced forty-five changes of government, hundreds of changes within the cabinet, countless military rebellions and pronouncements, a succession of foreign invasions, and wholesale territorial dismemberment. When this mix is leavened with a political system that produced a monarchy, a federal republic, a centralist republic, liberal reform, conservative reactions, and finally dictatorship, it is little wonder that few historians have ventured into the murky morass. One notable exception has been Michael Costeloe of the University of Bristol. An indefatigable researcher, Costeloe has explored in previous studies the inner workings of the ecclesiastical bank, the role of the Catholic Church in promoting a conspiracy during the Mexican-American War, and the process of tithe collection in the Archbishopric of Mexico. Now he has turned his attention to a study of the chaotic political party system that emerged in the fifteen years immediately after independence in 1821.

With contemporary newspapers as its principal source, this dense narrative indicates that all the permutations were in fact carried out within a small political elite with little or no popular participation. The elite was divided into three categories. The first, or upper elite, included the traditionally well-to-do: high clergy, wealthy landowners, high-ranking military, bankers, rich merchants, Spaniards, and a few European-oriented creoles. The middle level elite merged intellectuals, professionals, smaller landowners, and generally those favoring federalism, a free press, and private property. The majority of political actors, Costeloe's lower elite, was made up of the upwardly aspiring: deputies, senators, colonels, lower clergy, small businessmen, and a few artisans. From the ranks of these three groups came the bewildering array of political parties: Iturbidists, Bourbonists, centralist republicans, federalist republicans, the York-rite masons, the Scottish-rite masons, *novenarios*, *imparciales*, *aristócratas*, *hombres de bien*, liberals, conservatives, and many others.

Obviously this was a system of nonideological politics with individuals frequently shifting positions, parties, and loyalties. The most notorious of these shifters was Antonio López de Santa Anna, but Costeloe argues correctly that the seemingly mercurial behavior of Santa Anna was no different from that of most participants. What Costeloe does not explain is why this type of political behavior emerged after independence. He suggests in the concluding chapter that the military is the key to understanding that the problems Mexico faced transcended political structure. If so, then a systematic study of the Mexican army should have formed a fundamental part of the book. By not including this kind of analysis Costeloe avoids sticky questions about the role of the generals, the problems of political legitimacy, and the ex-

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planations of why the events unfolded the way they did. The result is an impressive and informative description of political chaos, but one that raises more questions than it answers.

RICHARD SINKIN
*University of Texas, Austin*

RAMÓN EDUARDO RUIZ. *Labor and the Ambivalent Revolutionaries: Mexico, 1911–1923*. Baltimore: Johns Hopkins University Press. 1976. Pp. 145. $10.50.

How "revolutionary" was the Mexican Revolution? Not very, if it was at all, Ramón Ruiz argues, joining those revisionist scholars who have been hard at work casting out Mexico from the pantheon of revolutions. This is largely an ideological judgment, but, to his credit, Ruiz does more than engage in forays into socialist moralizing. He has produced a first-rate study of Mexican labor from 1911 to 1923 which is based on extensive use of Mexican primary and newspaper sources.

Ruiz sets the stage with a chapter on the rise of labor activity and consciousness during the *Porfiriato*. In this discussion he concentrates on working conditions and strikes. In the next two chapters, dealing with the regimes of Francisco Madero and Victoriano Huerta, Ruiz argues that, "Paradoxically, in its [sic] willingness to recognize labor unions, Huerta's 'conservatives' broke with Porfirista political tradition that the Maderistas had left undisturbed."

Subsequent chapters provide an analysis of the ambivalent relationship between the *Constitutionalistas* and the labor movement. Some of the important themes are the decision of the Casa del Obrero Mundial to support Carranza, the demise of the Casa at the hands of the victorious First Chief, the rise of CROM, the framing of the labor clauses of the Constitution of 1917, and the mixed attempts to implement these during the Obregón administration. Ruiz himself takes an ambivalent position in assessing blame for the failure of reform efforts to match the rhetorical promises of the *Constitutionalistas*. In places he argues for a "betrayal" of the workers' expectations, but in the last chapter he realistically notes the vast economic problems faced by Mexico: "Practical reformers had to live with the facts of life, often disregarding in the process old commitments to their labor backers. The alternative was anarchy." To this reviewer that seems to be a good postscript to most revolutions.

ROBERT FREEMAN SMITH
*University of Toledo*

RALPH LEE WOODWARD, JR. *Central America: A Nation Divided*. (Latin American Histories.) New York: Oxford University Press. 1976. Pp. 344. $12.95.

Hubert Howe Bancroft allotted two thousand pages to Central America in his history of the Pacific states. Eighty years later, the Modern Nations in Historical Perspective series permitted Mario Rodríguez less than one-tenth that number. Now, with another decade added, the Latin American Histories series has set aside nearly double the space afforded Rodríguez, though scarcely one-fifth of that used by Bancroft.

The occasional summary review of Central American history holds obvious rewards for the average reader. Authors like Rodríguez and Woodward, intimately acquainted with the region and genuinely interested in its people, provide meaningful analyses of the present through their examinations of the past, each one tuned to the insights and language of the moment for which it is prepared. Because these surveys are intended for a wider public—the present one omits footnotes entirely, and uses the technique of reiteration rather liberally—may the more scholarly reader safely ignore them? The answer is no.

Central America receives much more attention today than it did in the time of Bancroft. Indeed, Woodward's selective guide to the literature on Central America itself mentions 270 publications from the decade 1965–74. Far from providing the new substance Central American history needs, however, these miscellaneous items make almost mandatory the framework Woodward provides. The reconstruction of the past of the isthmus remains in a malleable stage. Its details need elucidation; its broad outlines clamor as insistently for theory. Perhaps the nuances of the Central American scene will not finally yield to the class analyses of Woodward any more than they have to the language of nineteenth-century liberalism. But for the moment class analysis, particularly in economic terms, will assist greatly in the effort to tackle the isthmian quandaries.

Woodward accentuates economic causation, though without insisting that it explains everything. Political independence brought bad feeling between two groups of moneyed families. The conservatives who broke up the union represented the aristocratic interests in opposition to the commercial class. Positivist neoliberals, allying themselves with foreign capital, dominated the scene after the 1870s. A middle sector has made itself felt only since the 1940s. Any student of Central America needs to comprehend this framework and its many details, never previously set forth so clearly. Only then may he question whether Marco Aurelio Soto, the enlightened ruler of Honduras (1876–83) and Manuel Estrada Cabrera, the incompetent despot of Guatemala (1898–1920), can

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ever be meaningfully categorized together. He may further ask why elsewhere in Spanish America conservatives were equated with centralists, and liberals with states-rights advocates, while in Central America the labels ran in contradistinction. Will all labels eventually become too confining and call for resolution again into individuals?

FRANKLIN D. PARKER
*University of North Carolina, Greensboro*

COLIN G. CLARKE. *Kingston, Jamaica: Urban Development and Social Change, 1692–1962.* (American Geographical Society Research Series, number 27.) Berkeley and Los Angeles: University of California Press. 1976. Pp. xi, 270. $25.75.

MICHAEL PAWSON and DAVID BUISSERET. *Port Royal, Jamaica.* Oxford: Clarendon Press. 1975. Pp. xv, 204. $20.75.

Michael Pawson and David Buisseret, in a masterful piece of historical research, have disproved the assumption that the 1692 earthquake which destroyed Port Royal also buried the records needed for reconstructing the city's history.

After setting out the geography of Port Royal, they marshal in massive detail the process of building Fort Cromwell. Records of deeds in the Jamaica Archives help the authors to present profiles of some of the earliest residents and plans of the city. Documents in the British Museum and the Public Record Office and selected letters in the Calendar of State Papers permit the authors to describe the activities of some of the colonists who were privateers, the methods of the Commissioners of the Admiralty Courts who lived in Port Royal, and the character of trade centering on Port Royal. The authors also do a great job in reconstructing seventeenth-century life and in listing archeological discoveries made so far.

There is a disappointment, however, for students of imperial history. For example, the chapters purporting to describe "The Consolidation of English Power," while crediting Mr. Pepys with creating a "sound . . . central administration," do not explain how Port Royal fit into the overall colonial design. Nonetheless, the authors' descriptions of the role of the naval officer in Port Royal, the activities of the ships while at anchor in the harbor, the duties of the coastal pilots and naval captains, and the shenanigans of buccaneer Bannister are fascinating.

The chapter on "The Economic Development of Port Royal" suffers from a similiar sacrifice of analysis for description. The interesting accounts of shipping tell us nothing about the economic linkages between Port Royal and the hinterland, about merchants' profits and their influence on trading patterns, or about the effect of the legal and illegal trade on economic choices.

In spite of these minor shortcomings, Pawson and Buisseret must be commended for their attempt to re-create a colorful part of Jamaica's history.

Colin G. Clarke's work on Kingston complements the study of Port Royal. Principally a geographer, Clarke has drawn upon a variety of disciplines in putting together his profile of Kingston, the largest city in the non-Hispanic Caribbean. His 106 pages of tables and maps and 32 photographs, together with a bibliography containing most of the major literature on the Caribbean written before 1962, will prove useful. The book will be an indispensable reference. Four defects, however, affect the value of the study.

First, it is difficult to identify Clarke's central thesis. He insists that there are two. The first theme, he explains, is to analyze "the city's spatial, demographic, and economic growth" and to explore "the relationship between social and economic change." His "second" and "interconnected" theme "refers to the social structure of the city and in particular, to the changing relationship between race, culture, and status." In fact, these are not two, but several themes, and his central argument does not emerge coherently.

Second, *Kingston, Jamaica* suffers from methodological uncertainty and disciplinary obtuseness. A chapter on "Urbanization of Kingston on the Eve of Independence," for example, is similar in format to one entitled "Social and Economic Change, 1944–60"; and this chapter in turn repeats in substance another dealing with "Social and Economic Change, 1820–1938."

Third, he is obscure in his assignation of a "coloured race" and in delineating the characteristics making for cultural distinctiveness of the "coloured" group. How brown must a Jamaican be to be identified as belonging to that "race"? Where does one place the offspring of intershade or interracial mating? Do black civil servants and professionals belong to the colored race or the "significant others"? These questions are not discussed.

Fourth, Clarke's observation that "outstanding among these [psychological reactions] are the Negro's tendency to blame others for his misfortunes" is loaded with pathological inferences.

Some comparative studies would have assisted Clarke in getting rid of some of his negative perspectives. For example, recent studies by Joyce Ladner on black women in America and by Frances Henry and Pamela Wilson on the Caribbean explore positive role-identifications in female-headed families. Clarke's perspectives on the social and political impact of religion in black communities might have been enhanced by the

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works of Gayraud Wilmore (for the United States) and Leonard Barrett (for the Caribbean).

The defects in the book leave the field open for a more careful work on Kingston (and other capital cities in the Caribbean). Undoubtedly Clarke's work will spark further research in Caribbean urban history.

ENA L. FARLEY
*State University of New York, Brockport*

CHARLES D. AMERINGER. *The Democratic Left in Exile: The Antidictatorial Struggle in the Caribbean, 1945–1959*. Coral Gables, Fla.: University of Miami Press. 1974. Pp. 352. $19.95.

Charles D. Ameringer holds that the period 1945–1959 has unity for his subject in that a unique generation of international leaders collaborated for the realization of democracy, social justice, and economic progress in the Caribbean. Despite their sacrifices, success largely eluded them. This solid and sympathetic work, exhaustively documented, is a tale full of horror and heroism, idealism and sordidness, and frustration relieved by rare victories in the gangster politics of the area.

Emphasis falls on Acción Democrática of Venezuela, the Auténticos of Cuba, José Figueres and the Caribbean Legion in Costa Rica, and the Guatemalan left, with briefer treatment of lesser-known figures who tried to overthrow dictatorial regimes, sometimes in pitifully botched invasions. The Democratic Left believed that it was close to sweeping away the tyrants just after the end of World War II. But Costa Rica was its sole victory, thanks to the Caribbean Legion, whose controversial nature and role are clearly defined. The promising phase ended as the Cold War hardened United States commitment to stability and not revolution. Venezuela, then Cuba, and finally Guatemala succumbed. Costa Rica was largely neutralized by its duel with Nicaragua, where the Somoza dynasty survived, as did the Trujillo regime in the Dominican Republic.

The exiles persisted in harassing the dictators despite persecution, including torture and murder. The Inter-American Association for Freedom and Democracy led by Frances Grant, the C.I.A. at times, the free trade-union movement, and the Inter-American Press Association inspired the exiles and helped to undermine the dictators, while extracting occasional concessions from them. The author insists that the Democratic Left generally rejected Communist collaboration.

Acción Democrática was able to profit from the fall of the Pérez Jiménez regime in Venezuela. But the Auténticos lost out in Cuba to Fidel Castro, who challenged the credentials of the Democratic Left and broke with it everywhere. Perhaps the cause had been too often defeated. In the 1960s it would win few victories, and so far in this decade, fewer still. This book deserves a sequel.

JOHN E. FAGG
*New York University*

C. HARVEY GARDINER. *The Japanese and Peru, 1873–1973*. Albuquerque: University of New Mexico Press. 1975. Pp. xiii, 202. $12.50.

Few historians have given attention to the development of Asian interest in Latin America. The most recent contribution to this area is C. Harvey Gardiner's *The Japanese and Peru, 1873–1973,* which traces the development of diplomacy, immigration, trade, investment, and technical assistance between the two countries.

Accidental encounters characterized the first quarter-century of Japanese-Peruvian relations. On two occasions, in 1868 and 1872, ships carrying Chinese coolies to Peru terminated their voyages in Japanese ports, incidents which led the countries to open negotiations. Throughout the ensuing period Japan conducted diplomatic relations with Peru via its missions in Mexico and Chile. The latter contact was particularly galling to Peruvians who retained intense resentment for their southern neighbor after the War of the Pacific. Not until 1921 did Japan send a resident minister to Peru.

Even before the exchange of missions, however, Japanese emigration to Peru had brought the countries into closer and often acrimonious contact, a situation which continued through the 1940s. Preferring Europeans, but unable to attract them in sufficient numbers to work the coastal plantations, Peru accepted some twenty-eight thousand Japanese between 1898 and 1936. In analyzing the Japanese colony Gardiner relies almost entirely on reports from the Japanese Ministry of Foreign Affairs. His several tables detailing the numbers and characteristics of immigrants are commendable and could be improved only by adding data from the 1908 census for Lima and 1920 census for Callao. Virulent attacks by the Peruvian press on Japanese immigrants heightened the general Peruvian hostility toward them. The greatest blot on Peru's relations with the immigrants was the deportation of over fifteen hundred Japanese nationals and their *nisei* children to camps in the United States after the attack on Pearl Harbor. Those spared this uprooting often suffered loss of property without compensation. After the war the Peruvian government exacerbated the bitterness by refusing to allow many deportees to return. Gardiner clearly illustrates the harshness of these

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actions by using case studies from the United States Department of Justice.

In the postwar period relations between Japan and Peru shifted to mutually rewarding endeavors. Peru's growing desire to industrialize brought offers of investment capital and technical assistance from Japan. At the same time trade between the two nations increased and each promised the other most-favored-nation status. Despite improved relations, Gardiner concludes on a pessimistic note questioning the nations' continued compatibility given Peru's "revolutionary" government after 1968, inflation, and the fuel crisis which might necessitate curtailing trade across the Pacific. The first of these concerns may disappear as a result of the government's recent shift to the right, although given the long-standing uncertainties of Peruvian politics it is always chancy to make any predictions.

Gardiner relies heavily on Japanese sources. His references to Peruvian newspapers are usually to clippings found in the Japanese Foreign Ministry files. Had the author consulted Peruvian sources directly, he might have been able to offer a more well-rounded account. Furthermore a study of residential guides and commerical directories available for Lima and Callao would have enabled him to comment in greater detail on occupational and residential patterns of the Japanese colony there. These few weaknesses are exceptions, for Gardiner has written a well-balanced and valuable work which illuminates an important aspect of Peruvian history.

JANET E. WORRALL
*University of Northern Colorado*

FRANK SAFFORD. *The Ideal of the Practical: Colombia's Struggle to Form a Technical Elite.* (Latin American Monographs, number 39. Institute of Latin American Studies, University of Texas at Austin.) Austin: University of Texas Press. 1976. Pp. xv, 373. $15.00.

There are two books within this book. One, an overview of Colombian social history in the nineteenth century, had to be written because no one else has done that job. The other, and the central purpose of the work, is an evaluation of how well Colombian policy on technical education succeeded in achieving the goal of modernization. The first is largely unexceptionable and will serve well those students and nonspecialists seeking to know more about Colombia between Bourbon and "modern" times. This review is restricted to the contribution designed for specialists.

International organizations nowadays regularly send forth teams of experts to evaluate the success of agricultural, educational, and road-building projects. Armed with cost-benefit analyses and evaluation manuals, they try to tot up the pluses and minuses to see whether the recipient country is better (or worse) off for the international loans and grants received. All too often, too little time has passed to permit fair evaluation of a project. Frank Safford has a far greater time perspective than contemporary evaluators; that perspective makes up for the limitations of data and the imprecision of the original objectives of the Colombian policymakers.

The goal of the Colombian technical elite was to modernize, or, more precisely, to follow the example of the United States and Europe. The means of achieving their goal included travel by the elite themselves, importation of foreign teachers, training of the young in the U.S. and Europe, and the integration of these foreign-trained Colombians into the educational system after their training abroad. All these techniques were costly, and all of them failed to one degree or another. Many students left engineering for politics and commerce, both of which were more lucrative and interesting to the technical elite.

Despite his passionate interest in the period and the people, Safford maintains admirable objectivity throughout his monograph. He ably masks a sense of disappointment that weaves subtly through his work, disappointment that more was not accomplished. But the cause of Colombian backwardness resides in large measure in geographical and social obstacles that technical training and entrepreneurial effort alone could not overcome.

Fortunately, Safford compensates for the shortcomings of his subjects by his own energetic prosecution of the task. He has amassed a large number of data on individuals in three appendices and provides in his notes and bibliography an excellent guide for further work on the topic.

WILLIAM PAUL MCGREEVEY
*Washington, D.C.*

NEILL MACAULAY. *The Prestes Column: Revolution in Brazil.* New York: New Viewpoints. 1974. Pp. xi, 281.

The final decade of Brazil's "Old Republic" (1889–1930) was marked by political strife and military revolt. The most famous instance was the rebel army march through the Brazilian backlands in the mid-1920s. The column came to be identified with Luís Carlos Prestes, a young officer who later became one of Latin America's most durable Moscow-line Communist leaders.

Macaulay draws on his own experience as a combatant in Fidel Castro's guerrilla movement to describe sympathetically the practical problems

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faced by the rebel column—poor trails, shortage of cattle to slaughter, the constant threat of desertions and betrayal. He shows a sure hand in describing the principal personalities and in giving incisive sketches of the local political scene as background to the column's move through each region. The style is compact, lively, and colorful.

Macaulay convincingly argues that the rebels waged a "war of movement" aimed at Rio de Janeiro. It was therefore not a campaign with the Maoist aim of mobilizing rural support for the rebels' political cause, but rather a move within the context of intraelite political struggles. As such, it was one of the most important chapters in the story of military rebellion against the "elected" Republican politicians. Whether out of cunning, foresight, or sheer lack of resolution, the authorities in Rio refused to be drawn into an all-out attempt to annihilate the rebel column, which finally crossed into exile in Bolivia.

The author has relied primarily on printed memoirs and interviews, supplemented by materials in the archive of the Ministério do Exército, the Horácio de Matos archive, and the U.S. consular reports. Macaulay has chosen to give a carefully crafted "true story of the march," not an analysis of the legend of the Prestes Column, much less a socioeconomic or ideological analysis of *tenentismo*. The latter task remains to be tackled and can only result from a probing examination of the way in which the relationship between socioeconomic forces and political trends interacted with military discontent during the Old Republic.

THOMAS E. SKIDMORE
*University of Wisconsin, Madison*

TULIO HALPERÍN-DONGHI. *Politics, Economics and Society in Argentina in the Revolutionary Period*. Translated by RICHARD SOUTHERN. (Cambridge Latin American Studies, number 18.) New York: Cambridge University Press. 1975. Pp. xii, 425. $42.50.

Normally a reviewer defers his comments on stylistic matters to a less significant place than the opening sentence; but since the prose style of this recent volume in the Cambridge Latin American Studies series may discourage its potential readers, the issue of style should be confronted early. Stated flatly, the involved and convoluted sentences, the page-long paragraphs, the penchant for using double negatives, the almost deliberate avoidance of the simple declarative sentence, all make this a difficult book to read and comprehend. The danger exists, then, that those who might well benefit from its close examination will be alienated from doing so. This would be most unfortunate, for Tulio Halperín-Donghi has successfully undertaken a major task: the reinterpretation of the history of the entire era that saw the transformation of the River Plate area from the late eighteenth-century Bourbon viceroyalty to the politically independent but largely inorganic United Provinces of the 1820s.

The book is organized into three parts. The first is a background section which examines in detail the economy of the area and the changes produced initially by the commerical reforms of the Bourbons and then by what the author calls the prejudicial consequences associated with the revolutionary process. The second part, which constitutes the core of the book, analyzes the political and social changes accompanying the breakdown of the colonial order prior to 1810, the evolution of the revolution in Buenos Aires and the country as a whole, and the dissolution of the revolutionary order after 1819. The final part examines the legacy of the revolution and of the war in terms of their impact on political life in subsequent decades.

Throughout the volume the author is concerned with the processes of change, with uncovering the forces at work that shaped the revolutionary era. His is no simple or deterministic explanation. Unlike Argentina's classic historians of the nineteenth century, Mitre and López, he does not assume that the development of the future Argentine nation with its specific boundaries was the necessary outcome or even the desired goal of those who took part in the events. Nor does he accept the views of twentieth-century historians who see only the economic dimension of political events. Complexity and fluidity are the features of the revolutionary process that the author seeks to disentangle by exploring the changing relationships within and among the political, social, and economic elites, not only in Buenos Aires, but in the interior as well.

Indeed, one of the most valuable aspects of this volume is the attention paid to the history of the interior, which is viewed not as a homogeneous entity, but as a group of distinct communities that contrast with one another as sharply as they do with Buenos Aires. In the process, the author challenges many long-established views such as the alleged fatal impact of the free-trade ordinance on craft industries, or the reasons for the emergence of the caudillos. It was the militarization and ruralization of Argentine society, changes attributable to the effects of the revolution, prolonged war, and involvement in the world market that basically undermined the effort to create a national power center in Buenos Aires and promoted the emergence of regional authorities in the person of the caudillos. The Argentina of Rosas, the author concludes, "was the legitimate heir of 1810."

Halperín-Donghi has made a significant contri-

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bution to the literature on Argentine independence. The publisher should explain, however, why, given the astronomical price charged for the book, it does not include at least one map and the comprehensive index it deserves.

ROBERT A. POTASH
*University of Massachusetts, Amherst*

ALBERTO J. VARONA. *Francisco Bilbao, revolucionario de América: Vida y pensamiento, estudio de sus ensayos y trabajos periodísticos*. Panama City: Ediciones Excelsior. 1973. Pp. xxix, 457.

This book brings together details about a Latin American thinker of the early nineteenth century in whose peripatetic life the diverse intellectual strands of the young republics were woven. Varona divides the book into three parts—one covers Bilbao's activities, one his essays, and another his journalistic writings—in order to examine his angle of vision. He reminds us of the competing loyalties that tugged strongly at Latin Americans in that era, and he deftly singles out a particular Latin American offering to the intellectual history of the Americas: a Roman Catholic, utopian socialist outlook.

This viewpoint had its origins in a social ambience dominated by the clergy. In this search for a cure Bilbao was attracted to the French abbot, H. F. R. de Lamennais, whose critique of the Church was intended to rejuvenate it. Lamennais took him on as a student, and thereafter Bilbao sought to convince Latin Americans that the Church must be destroyed or it would undermine republicanism.

Bilbao was an educational radical, not, as the subtitle claims, a revolutionary. He argued that churchmen would not foster education's proper goals—social equality and universal freedom, notions he found eminently compatible. Underneath his rabid anti-clericalism lay an equally strong conviction that the Church's former otherworldly asceticism was worth emulating. Nevertheless, he was a founder of the *Sociedad de Igualidad* and he participated in an abortive 1851 uprising. Varona thinks the *Sociedad's* effort to satisfy the needs of workers and professionals frustrated the rise of a middle-class ideology of reform in Chile and ended in the formation of working-class parties in the twentieth century.

In exile Bilbao formulated the cultural theory that gave him lasting fame. His attacks upon the persistence of medieval tradition led to his denunciation of European imperialism and the chattel condition of women, Africans, and Asians. The United States puzzled him. Aghast at the crass materialism he knew represented the ultimate danger to Latin America, he simultaneously admired American individualism. To resolve this problem he declared that Emerson and the Unitarians were the best America could offer her neighbors. Ultimately, however, Bilbao expected that the most effective weapon in the fight against these evils was the "Latin genius" for generosity born of respect for human dignity. Whereas Sarmiento, his Argentine contemporary, placed his trust in the collective will of society, to Bilbao the individual was divine.

Varona has combed the sources to review the Chilean's contribution for us in craftsmanlike style. This is the best study of Bilbao since the work of Armando Donoso (1913); it also helps to clarify a poorly understood period in Latin American intellectual history.

VINCENT C. PELOSO
*Howard University*

FREDRICK B. PIKE and THOMAS STRITCH, editors. *The New Corporatism: Social-Political Structures in the Iberian World*. (International Studies of the Committee on International Relations.) Notre Dame: University of Notre Dame Press. 1974. Pp. xxii, 218. Cloth $8.95, paper $2.95.

Corporatism as an analytical tool was revived in the mid-1960s by a few writers working on Latin America to replace the vacuous concepts of "democracy," "totalitarianism," and "modernization." In examining various efforts at capitalist development in Peru and Chile, for example, I observed unsuccessful attempts at corporatist implementation under several regimes. Some of the essays in this collection have taken the whole discussion a step further and attempt to explain much of what occurs in the "Iberian" world as a product of this phenomenon (Wiarda and Newton). Others have extended the notion of corporatism to explain European political development (Schmitter), while one author finds the corporatist prescriptions an appropriate formula for Iberian development (Pike), and another provides a descriptive inventory of corporatist features in Peru (Malloy). One cannot argue with this renewed interest in corporatism, only with the quality and substance of the discussion.

What is striking about corporatism is not its frequent articulation, emergence, or reemergence, but its dissolution and decay as a political form. The grand schemes of functional representation remain formal shells when they leave the drawing board. The recent "discovery" of corporatism in Ibero-America and elsewhere by U.S. academics has led them to stretch the meaning of the term to cover a great variety of heterogenous forms of association, reaching back in history to a number of

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earlier authoritarian political traditions in order to give larger meaning and significance to the discussion. Rather than adding to our knowledge of corporatism, this approach weakens the concept's specific meaning—forcing "corporatism" to be a catch-all phrase describing any set of state-defined relations.

No doubt numerous "corporatist" ideas, agencies, and ideologies have emerged from time to time and planted a seed here and there, but what is curious is the lack of any substantial ongoing experience showing the harmony of labor and capital, which is central to the ideology. Both the corporatist ideologies and their analysts (unfortunately at times one is hard-pressed to separate them) consistently underplay the importance of force and violence in establishing the basis of corporatist states. Moreover, there is little analysis of the social forces or coalitions which bring corporatist political regimes to power and which are dissolved once the regimes are instituted. Why that should occur is not even discussed.

Schmitter locates the phenomenon beyond the confines of the Iberian world, positing the existence of corporatism as a worldwide phenomenon and thus avoiding the crude "national character," "Latin tradition" pseudo explanations. Anchoring corporatism in diverse capitalist societies requires a general explanation which identifies the historical conjuncture within which corporatism can be explained. Schmitter provides an inadequate theoretical explanation which fails to elaborate on the rise of European corporatism as a specific response to the breakdown of capitalism, a crisis of the political regime (usually parliamentary), and an effort to atomize class-anchored workers' movements and reintegrate them into the political-economic project of the capitalist class.

The nature of the impact of imperialism on political and social structure is obscured in Pike's essay on U.S.–Latin American relations. He assumes that something called the "internal dynamic" of Latin societies takes precedence without observing that the whole growth cycle of the region was and continues to be conditioned by outside demands (mostly American); and that foreign exchange, loans, military/police training, and a whole host of other "variables" shape the political structures. The interesting question then would be what impact does this transparent foreign presence have in shaping, modifying, or undermining corporatist pretensions? These essays hold to an ancient and inadequate paradigm—that the independent nation-state rather than the world capitalist system is the basic unit of analysis.

No sense of historical specificity is displayed in the use of "corporatism." Whether one examines Wiarda's wandering over time immemorial or Schmitter's meandering in Europe, one sees corporatism applied to a great variety of regimes and practices in a way seems to explain everything and explains nothing in particular. A theory of Latin American corporatism must, at a minimum, locate the political form in a conjuncture, anchor it in the class structure, and trace out its historical evolution within the wider universe of interstate relations.

JAMES F. PETRAS
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# Communications

*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

TO THE EDITOR:

In his review of my study on *Charles A. Beard and American Foreign Policy* (June 1976) Bruce Kuklick complimented the conscientious and diligent nature of the work, for which I am grateful. There were, however, some statements which call for critical commentary. For example, he contends that I have "implicitly contrasted" Beard's writings to "such State Department partisans as Herbert Feis, William L. Langer, and Arthur Schlesinger, jr." Moreover, in using quotation marks around the phrase "detached scholars," Kuklick conveys the impression that I referred to them as such in my book. Neither is the case. The former charge is based upon Kuklick's interpretation, not my intention. Further, on p. 76 I identified Feis as an economic adviser to the State Department. The other reference to Feis (p. 140n.) pointed out his considerable access to unpublished records, but also noted Feis' complaint about unnecessary restrictions on the ability of all scholars to examine certain government records. On p. 145 I characterized William L. Langer (along with S. Everett Gleason) as "pro-Roosevelt historians." Earlier Langer's study, *Our Vichy Gamble,* was described (p. 139) as "a defense of the Roosevelt administration's cooperation with the Vichy regime in North Africa in 1942." In that same passage, I indicated that Beard agreed with Langer's thesis and was "glad he did his work." As for Schlesinger, in a footnote on p. 155, his review of Beard's last book on FDR was cited along with three other reviews as representative of the pattern of criticism which greeted the work, not as the views of "detached scholars." In another footnote on page 126, Schlesinger was classified as a "foremost liberal historian" who, while critical of Beard's analysis of FDR's diplomacy, had recently (in *The Imperial Presidency*) "echoed Beard's complaint about the ways in which executive authority has been exercised" in the conduct of American foreign policy.

Kuklick also suggests I did not examine the possible cause and effect relationship "between the events of 1939–1949 and the events that terrified Beard—the loss of civil liberties, the regimentation of thought, the growth of a monstrous military establishment, and the large-scale deliberate deception of the American people by a president of the United States." Granted, I did not write about such causal possibilities in depth—this would involve a book I did not propose to write. But my epilogue contains a brief discussion of how Beard's anxieties about such issues have struck many Americans as prophetic and valid.

Finally Kuklick faults me for presumably failing "to appreciate the central importance of Beard's belief that American involvement in war was destructive of reform at home," and for "dismissing his understanding of the link between foreign and domestic policy." I would submit, however, that Kuklick dismisses the importance of my summary statements on these themes found on pp. 163–64. In the text, moreover, there are instances too numerous to cite or quote when Beard's concerns about war and domestic reform and his reflections on the link between foreign and domestic policy were thoughtfully considered. I would simply offer the following example for comparison with Kuklick's critique: "With each act of the Roosevelt administration which further involved the nation



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in the world's political and economic problems, and which was accompanied by an increase in defense expenditures, Beard became more and more convinced that Roosevelt was betraying his own program of domestic reform and recovery. Such policies were viewed by Beard as prima-facie evidence that FDR was seeking war as a means of deflecting public attention from his failure to solve the obvious human suffering of the continuing Great Depression" (p. 77).

THOMAS C. KENNEDY
*University of Wyoming*

TO THE EDITOR:

I have read Thomas C. Kennedy's letter and have reread both *Charles A. Beard and American Foreign Policy* and my review. I see no reason to change what I have written about his book. I advise readers themselves interested in the issues carefully to study the book and my review.

BRUCE KUKLICK
*American Quarterly*

TO THE EDITOR:

Your June issue contains George M. Fredrickson's review of my book *Inventors of the Promised Land*. Fredrickson clearly understands and sees "a measure of truth" in the basic argument of the book on spread-eagle patriotism in America and its racial and sexual offshoots between 1786 and 1840. Beyond that, however, I "load the dice" in my "selection of individuals, incidents, and quotations" to sustain my argument.

First, Fredrickson maintains that I limit my "discussion of the early prophets of American nationality almost entirely to the Federalist or conservative school of thought" and ignore Jeffersonians. Yet my two chapters on general patriotic attitudes in the early national period—one on contributors to the *Columbian Magazine* and the other on formulators of the mythic George Washington—deal with a wide range of political and ideological types. Several Jeffersonians (Jefferson among them) contributed to the *Columbian*. The Washington myth was hardly the exclusive creation of "the Federalist or conservative school." A diverse array of Jeffersonians (including Jefferson) participated in the myth-making process. Finally, two of the three figures that I treat at length early in the book who usually voted Federalist—David Ramsay and Charles Brockden Brown—displayed neither consistently strong partisanship nor consistently conservative ideas. But my book, like other studies of early American patriotic thought, does cite more materials by Federalists than by Jeffersonians. The real issue is whether party affiliation revealed significant differences in spread-eagle patriotic articulations. Like most analysts of early patriotic thought, I simply did not detect major differences that ran consistently along party lines. Fredrickson may properly challenge this finding.

Second, Fredrickson charges that I "load the dice" when I write that "Garrison and other radical abolitionists remained covert colonizationists" and when I then go on to say that "slavocrats of the deep South" fit into this "colonizationist consensus." This is not quite the case, for I emphasize that abolitionists could in no way be equated with slavocrats (see, for example, p. 253, "A fundamental and humanistic commitment to universal emancipation and civil rights cannot even remotely be equated to the defense of Negro slavery . . ."). From 1830 to 1834 Garrison remained ambivalent about colonizing blacks in Africa, while Benjamin Lundy and James G. Birney occasionally made procolonizationist statements after 1834. My point was that certain abolitionists were, for a time and on occasion, willing to entertain the option of colonizing blacks abroad in the event that controls of the slave system eroded. But this is not even to imply that abolitionists and slavocrats were in fundamental accord.

These qualifications aside, I thank George M. Fredrickson for his provocative comments.

LAWRENCE J. FRIEDMAN
*Bowling Green State University*

TO THE EDITOR:

Despite Lawrence J. Friedman's thoughtful and courteous response, I stand by the opinions I expressed in my review. I do indeed think there were significant ideological differences between Jeffersonians and Federalists which implied somewhat divergent conceptions of American nationality. I of course concede Friedman's right to a contrary opinion, but I do not find this viewpoint supported in his book by an adequate examination of anti-Federalist thought. As I pointed out in the review, he himself absolved Jefferson from responsibility for endorsing much of the mythology he describes.

On the question of a "colonizationist consensus," our disagreement is partly a question of fact and partly a matter of definition. I understand that Friedman is not guilty of the absurdity of equating abolitionists and slavocrats in terms of their total ideological commitments. Yet he does appear to be saying that they shared a common hope or aspiration for the elimination of blacks from American society. If this is indeed the thrust of his argument, then he is giving a misleading impression of both groups. The embodiment of the colonizationist philosophy of racial homogeneity

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was the American Colonization Society. In the 1820s most of the slavocrats of the deep South rejected this movement and its ideology from a proslavery perspective. Abolitionists renounced it in the early 1830s because it was contrary to their antislavery egalitarianism. I do not find Garrison and other militant abolitionists the least bit ambivalent about the aims and philosophy of the American Colonization Society. For them, rejection of colonization was in fact virtually synonymous with the acceptance of abolitionism. But an abolitionist commitment did not preclude occasional sympathy for stopgap proposals for black emigration that were distinct in sponsorship, ideology, and methods from the efforts of what I take to be the colonizationist movement.

GEORGE M. FREDRICKSON
*Northwestern University*

TO THE EDITOR:

Fritz Lehman erred in his review of my book, *Vilyatpur, 1848–1968: Social and Economic Change in a North Indian Village* (*AHR*, 80[1975]: 1031–33), when he stated that "Kessinger married into the Vilyatpur landowning caste—which one suspects gave him unique advantages in access to family and pilgrimage records." While my wife is an Indian and a Punjabi, she is neither from *the* village nor any village; nor does she belong to any of the castes in the community. I make this correction not because we are offended (most Punjabi groups have been trying to pass for Jats for over two hundred years), but because of the extraordinary qualification it suggests for access to the documents I used. The fact is that the revenue, land title, legal, and census records on which the study is based, and which have been ignored by historians to date, exist for thousands of north Indian villages and are available on application to the appropriate government offices. One of my purposes is to call attention to these records emphasizing their use for family and demographic history comparable to that done in Europe and the United States.

TOM G. KESSINGER
*University of Pennsylvania*

TO THE EDITOR:

Despite the disparaging terms Felix Gilbert uses, I would not normally have made any statement concerning his review (June 1976), as I am confident that the reader will easily recognize the contradictoriness of a critique which charges me with overvaluing ideas and, simultaneously, with denying "the impact of moral considerations, emotions, and values in political life," which tries to prove my "Hegelianism" by criticizing my inference from Bolshevism to Fascism (an inference not different in character from an inference from the industrial revolution to the workers' movement), and which, at the same time, objects to my "nationalism" and to a "fanaticism of objectivity." But I must strongly protest against Gilbert's citations on p. 620 and the imputations they contain.

Gilbert gives it as my opinion, that "the only difference between Auschwitz and the American conduct in Vietnam was that the Americans could protest, the Germans could not." My text on p. 528 reads as follows (in my translation): "With the full sensorium of the welfare society America perceived the worldwide protests reproaching the United States putting into practice in Vietnam after all nothing less than their more cruel version of Auschwitz. Yet, in contrast to the Germans during World War II, many Americans were willing and in a position to protest. As early as 1965 protesters burned themselves in front of the Pentagon. . . ."

He does not even cite a page when saying that, for me, anti-Semitism is a by-product of capitalism, and he would not be able to do so, indeed, as I strongly underline Stalin's anti-Semitism. However, I am not too astonished to see myself placed into the wrong environment, because Gilbert, in the penultimate paragraph of his review, reprimands me with having overlooked a fact which, indeed, is fundamental for the concept of my book, namely that Germany was but a part of a global conflict and that the acknowledgment of the partition was a consequence and not the cause of the Cold War entering into its "latent phase." Therefore the question might be unavoidable whether this review conveys at least an adequate impression of the contents, the structure, and the intentions of *Deutschland und der Kalte Krieg*.

ERNST NOLTE
*Freie Universität Berlin*

TO THE EDITOR:

I characterized Ernst Nolte's book as "a Hegelian history in which events are determined by the logic of concepts"; I criticized the book because the author failed to recognize the impact of "interests and personalities, emotions and miscalculations, passions and moral considerations." I find no contradiction between characterization and criticism.

I find Nolte's statement on p. 528 as objectionable in English as in German. Although I deeply regret and condemn the inhumanities committed in Vietnam, I cannot consider the events in Vietnam as a worse version of Auschwitz. The murder of six million people in the holocaust was a systematically planned action ordered and supervised by the heads of government and carried out by a large

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bureaucracy organized for this particular purpose. In concept and effect this was different from what happened in Vietnam. Moreover, I question Nolte's suggestion that—like the Americans—the Germans would have demonstrated against the barbarism of the Nazi government, if they had not been prevented from such action by the rigors of wartime controls. There were no public protests in Germany even at times when such demonstrations might still have been possible.

On p. 159, Nolte calls the "*Vernichtung der europäischen Juden*" a "*Versuch, Probleme, die mit der Industrialisierung zusammenhängen, durch die Beseitigung einer grossen Menschengruppe zu lösen*"; he assumes a connection between modern economic developments and anti-Semitism. Of course, such connection is indispensable. In my review, however, I was concerned not with this issue but with Nolte's tendency to mitigate the specific virulence of Nazi anti-Semitism by directing attention at every possible moment to the appearance of anti-Semitism in other countries and in every modern society (see, for instance, on p. 160 the remark that Roosevelt and Hopkins would have sympathized with anti-Semitism if a strong Communist party had existed in the United States).

Nolte's objection to the penultimate paragraph of my review is hard to grasp. But I fully agree that there are fundamental differences between us regarding Germany's role in the Cold War. He regards the "*Ostverträge*" which the German Federal Republic concluded to have been a decisive action which terminated the Cold War and prevented the transformation of the Cold War into a hot war. See, for instance, the statement on p. 598: "*Zum erstenmal in der ganzen Weltgeschichte war ein 'transzendentales Interesse' der Menschheit im Spiel, und Deutschland beugte sich durch die Stimme der Bundesrepublik dem obersten aller möglichen Imperative.*" I believe that Nolte overestimates the importance of the *Ostverträge* and that at the time of the *Ostverträge* the danger of an explosion—if it had ever existed—no longer existed. Nolte's presentation of the end of the Cold War is particularly interesting because it reveals clearly a main tendency of the entire book: "*Deutschland wieder in die Mitte der Welt zu rücken und die Deutschen zur Anerkennung der zentralen Bedeutung ihrer Geschichte zu ermutigen.*" This formulation can be found in the sixteen-page-long review which was published in the *Historische Zeitschrift* (vol. 221 [1975]) and which I saw only after I had sent in my review. This essay is much more critical of Nolte's book than I have been in my review.

FELIX GILBERT
*Institute for Advanced Studies*

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# Recent Deaths

ROBERT F. KELLEY, who some fifty years ago trained a whole generation of Foreign Service officers for duties relating to the Soviet Union and Western Europe, died on June 2, 1976, in Washington, D.C., after a long illness.

He was born in Somerville, Massachusetts, February 13, 1894, the son of John Henry and Carolina Martha (Miller) Kelley. After graduation from the West Roxbury High School, he received an A.B. degree cum laude from Harvard University in 1915 and an A.M. Degree in 1917.

While still in high school Kelley became interested in matters relating to Russia, and as he pursued his studies at Harvard he specialized in Russian history, literature, culture, and language. Following graduation he spent several months on research relating to Russia at the University of Paris. When the situation in France as the result of the First World War made further research there difficult, he returned to Harvard as an instructor in history.

Following the entrance of the United States into the war, he entered the Officers' Training Camp at Plattsburgh, N.Y., and upon graduation was commissioned as a second lieutenant, assigned to intelligence work. After serving in the occupied area in Germany, he was appointed in March 1920 as military attaché to the American legation in Copenhagen and military observer, with residence in Riga, Latvia, to the Baltic States. In July 1921 he became military attaché to the United States legation in Finland and military observer to the Baltic States with continued residence in Riga—the chief American observation post for developments in the Soviet Union. Subsequently, he entered the Foreign Service and in 1922 was appointed vice consul to Calcutta.

In view of his specialized knowledge and experience in Eastern Europe Kelley was called back to the Department of State in June 1924 and assigned to the Division of Eastern European Affairs. Within two years he had become chief—the youngest chief of a geographic division of the department at that time. He continued to serve in that capacity for the next eleven years.

Although the United States did not have diplomatic relations with the Soviet Union until November 1933 and, as a consequence, had no representation in that country, Robert Kelley took the lead in the later twenties in preparing promising young Foreign Service officers for work in, or connected with, the Soviet Union. When relations were established, therefore, it was possible to staff the embassy in Moscow immediately with young officers acquainted with the Russian language, Russian history, and political and economic conditions in the Soviet Union.

During his years of service in the Department of State he was the trusted adviser on matters pertaining to the Soviet Union to no fewer than four secretaries of state—Hughes, Kellogg, Stimson, and Hull. He assisted in the drafting of the documents that were exchanged at the time of the establishment of diplomatic relations, and he played an important role in the organization of the new embassy in Moscow. The traditions of scholarship that he established in the department helped to prepare it to meet the problems of an international character which the United States has been called upon to face during the last forty years.

In 1937 Kelley left the Eastern European field to serve as deputy chief of mission and intermittently chargé d'affaires in the embassy in Turkey. At the end of World War II, he retired from the Foreign Service and became interested in assisting refugees from the Soviet Union, particularly those who were trying to obtain the freedom of friends and relatives still in their homeland. He participated in the work of a committee of American citizens which in 1951 established the short-wave station, Radio Liberty, so Russian emigres could talk to their countrymen in the Soviet Union. Following the opening of that station in Munich, Germany, he served as its chief for many years. After his retirement Robert Kelley resided for the last ten



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years of his life in Washington, D.C. He is survived by his sister, Mrs. John Burns of Jamaica Plain, Massachusetts.

Robert F. Kelley was a member of the American Historical Association, the American Society of International Law, the Washington Institute of Foreign Affairs, Diplomatic and Consular Officers of the United States, Retired, the Catholic Club, and Phi Beta Kappa.

LOY W. HENDERSON
*Washington, D.C.*

With the sudden death, on August 11, 1976, of ARTHUR WRIGHT, the field of Chinese historical studies lost one of its most creative minds and most effective builders.

Arthur Wright received his A.B. from Stanford in 1935, a B. Litt. from Oxford in 1937, and his Ph.D. from Harvard in 1947. He taught at Stanford from 1947 to 1959, at which point he came to Yale with his wife, the late Professor Mary Clabaugh Wright; and he remained at Yale from that year until his death in 1976. In 1961 he was named Charles Seymour Professor of History.

From the very beginning of his teaching career he saw the need to coordinate work on a number of fronts if Chinese studies were to continue to prosper in the United States. He was always active in the Association for Asian Studies, as editor of its journal for many years, as a director, and as president of the association; through the Committee on Chinese Thought of the association, and later as Chairman of the American Council of Learned Societies' own Committee on Studies of Chinese Civilization, he worked actively as coeditor on five symposium volumes, and helped encourage many more; and he was deeply involved with the Concilium on International and Area Studies at Yale, and with the Committee on International Historical Activities of the American Historical Association.

He was a great teacher because he loved his work, and that work was multifaceted and absorbing to his students. In the subtlety of his approach to the history of ideas, his sensitivity to the great philosophical traditions of China (which for him always included the Buddhist as well as the Confucian and the Neo-Confucian), in his complex historiographical studies of stereotypes, and his meticulous examinations of Chinese cities, he brought an amazing range of insights from other civilizations and other times. "I am implicitly comparative," he wrote in one of the last letters before he died. It was in the same spirit that he always sought for maximum range in his symposia—introducing, often for the first time to American readers, an absorbing array of scholars from Europe, China, and Japan.

Since I was lucky enough to be his colleague as well as his student, I came to understand his fairness and his shrewdness, and as his friend to value his vigorous integrity. He was not spared unhappiness, but he could handle it, both his own and that of others. He was a sweet, fine, intelligent man.

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*Yale University*

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# Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

H. C. ALLEN, and ROGER THOMPSON, editors. *Contrast and Connection: Bicentennial Essays in Anglo-American History*. Athens: Ohio University Press. 1976. Pp. ix, 373. $16.00.

DAVID GRAYSON ALLEN, A Tale of Two Towns: Persistent English Localism in Seventeenth-Century Massachusetts. ROGER THOMPSON, The Puritans and Prurience: Aspects of the Restoration Book Trade. R. C. SIMMONS, The Massachusetts Charter of 1691. C. C. BONWICK, English Dissenters and the American Revolution. ESMOND WRIGHT, The Loyalists. H. C. ALLEN, The American Revolution and the Anglo-American Relationship in Historical Perspective. MALDWYN A. JONES, Immigrants, Steamships and Governments: The Steerage Problem in Transatlantic Diplomacy, 1868–74. CHARLES S. CAMPBELL, Edward J. Phelps and Anglo-American Relations. VIVIAN VALE, Trusts and Tycoons: British Myth and American Reality. KENNETH O. MORGAN, The Future At Work: Anglo-American Progressivism, 1890–1917. DAVID MORGAN, Woman Suffrage in Britain and America in the Early Twentieth Century. ROBERT GARSON, The Atlantic Alliance, Eastern Europe and the Origins of the Cold War: From Pearl Harbor to Yalta. HOWARD TEMPERLEY, Anglo-American Images.

JOSEF L. ALTHOLZ, editor. *The Mind and Art of Victorian England*. Minneapolis: University of Minnesota Press. 1976. Pp. vii, 206. $15.00.

JEROME H. BUCKLEY, Victorian England: The Self-Conscious Society. ROBERT LANGBAUM, The Art of Victorian Literature. DAVID J. DELAURA, The Poetry of Thought. JOSEF L. ALTHOLZ, The Warfare of Conscience with Theology. JOHN M. ROBSON, Thoughts on Social Change and Political Accommodation in Victorian Britain. LEONARD G. WILSON, Science by Candlelight. MICHAEL R. BOOTH, Going on Stage. GEORGE P. LANDOW, There Began to Be a Great Talking About the Fine Arts. PHOEBE B. STANTON, Architecture, History, and the Spirit of the Age. MELVIN WALDFOGEL, Narrative Painting.

MICHEL ANTOINE *et al. Origines et histoire des cabinets des ministres en France*. (École Pratique des Hautes Études, Sorbonne. IV[e] Section: Centre de recherches d'histoire et de philologie. Series 5: Hautes études médiévales et modernes, number 24.) Geneva: Librairie Droz. 1975. Pp. 179.

JEAN FAVIER, L'entourage politique du prince au Moyen Age. HÉLÈNE MICHAUD, L'entourage des grands officiers et "les cabinets" des secrétaires d'Etat au XVI[e] siècle. MICHEL ANTOINE, L'entourage des ministres aux XVII[e] et XVIII[e] siècles. JEAN TULARD, La notion de cabinet ministériel sous l'Empire. JEAN VIDALENC, Les cabinets et entourages ministériels sous la Restauration. ANDRÉ-JEAN TUDESQ, Les chefs de cabinet sous la Monarchie de Juillet. PIERRE GUIRAL, Les cabinets ministériels sous le Second Empire. PIERRE BARRAL, Les cabinets ministériels sous la III[e] République. PIERRE LEGENDRE, Les cabinets ministeriels de la III[e] République après la Grande Guerre.

BERENICE A. CARROLL, editor. *Liberating Women's History: Theoretical and Critical Essays*. Urbana: University of Illinois Press. 1976. Pp. xiv, 434. $5.95.

ANN J. LANE, Woman in Society: A Critique of Frederick Engels. BERENICE A. CARROLL, Mary Beard's *Woman As Force in History*: A Critique. DOLORES BARRACANO SCHMIDT and EARL ROBERT SCHMIDT, The Invisible Woman: The Historian As Professional Magician. LINDA GORDON, PERSIS HUNT, ELIZABETH PLECK, ROCHELLE GOLDBERG RUTHCHILD, and MARCIA SCOTT, Historical Phallacies: Sexism in American Historical Writing. ANN D. GORDON, MARI JO BUHLE, and NANCY SCHROM DYE, The Problem of Women's History. HILDA SMITH, Gynecology and Ideology in Seventeenth-Century England. ADELE SIMMONS, Education and Ideology in Nineteenth-Century America: The Response of Educational Institutions to the Changing Role of Women. AMY HACKETT, Feminism and Liberalism in Wilhelmine Germany, 1890–1918. ROBIN MILLER JACOBY, Feminism and Class Consciousness in the British and American Women's Trade Union Leagues, 1890–1925. ANN M. PESCATELLO, Latina Liberation: Tradition, Ideology, and Social Change in Iberian and Latin American Cultures. JOYCE A. LADNER, Racism and Tradition: Black Womanhood in Historical Perspective. KAY BOALS, The Politics of Cultural Liber-



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ation: Male-Female Relations in Algeria. SARAH B. POMEROY, A Classical Scholar's Perspective on Matriarchy. KATHLEEN CASEY, The Cheshire Cat: Reconstructing the Experience of Medieval Woman. ASUNCIÓN LAVRIN, Women in Convents: Their Economic and Social Role in Colonial Mexico. ANN D. GORDON and MARI JO BUHLE, Sex and Class in Colonial and Nineteenth-Century America. RENATE BRIDENTHAL and CLAUDIA KOONZ, Beyond *Kinder, Küche, Kirche:* Weimar Women in Politics and Work. ALICE KESSLER-HARRIS, Women, Work, and the Social Order. GERDA LERNER, New Approaches to the Study of Women in American History. GERDA LERNER, Placing Women in History: A 1975 Perspective. HILDA SMITH, Feminism and the Methodology of Women's History. JULIET MITCHELL, Four Structures in a Complex Unity. SHEILA RYAN JOHANSSON, "Herstory" as History: A New Field or Another Fad?

WILLIAM C. JORDAN *et al.*, editors. *Order and Innovation in the Middle Ages: Essays in Honor of Joseph R. Strayer.* Princeton: Princeton University Press. 1976. Pp. xii, 582. $20.00.

FREDERIC F. KREISLER, Domesday Book and the Anglo-Norman Synthesis. GILES CONSTABLE, Cluniac Administration and Administrators in the Twelfth Century. JOHN B. FREED, The Friars and the Delineation of State Boundaries in the Thirteenth Century. CHARLES M. RADDING, The Administrators of the Aids in Normandy, 1360–1380. WILLIAM M. BOWSKY, Italian Diplomatic History: A Case for the Smaller Commune. PHILIPPE WOLFF, The Significance of the "Feudal Period" in the Monetary History of Europe. THOMAS N. BISSON, Credit, Prices and Agrarian Production in Catalonia: a Templar Account (1180–1188). RICHARD W. KAEUPER, Royal Finance and the Crisis of 1297. JOHN F. BENTON, The Accounts of Ceperello da Prato for the Tax on *Nouveaux Acquets* in the Bailliage of Troyes. ARCHIBALD R. LEWIS, Northern European Sea Power and the Straits of Gibraltar, 1031–1350 A. D. WILLIAM C. JORDAN, Supplying Aigues-Mortes for the Crusade of 1248: The Problem of Restructuring Trade. TEOFILO F. RUIZ, Castilian Merchants in England, 1248–1350. ROBERT S. LOPEZ, Proxy in Medieval Trade. RAPHAEL R. DESOIGNIE, The Fairs of Nimes: Evidence on Their Function, Importance, and Demise. ROBERT L. BAKER, The Government of Calais in 1363. RHIMAN A. ROTZ, Investigating Urban Uprisings with Examples from Hanseatic Towns, 1374–1416. BENNETT D. HILL, The Counts of Mortain and the Origins of the Norman Congregation of Savigny. FREDRIC CHEYETTE, The Castles of the Trencavels: A Preliminary Aerial Survey. JAN ROGOZINSKI, Ennoblement by the Crown and Social Stratification in France 1285–1322: A Prosopographical Survey. BRUCE MCNAB, Obligations of the Church in English Society: Military Arrays of the Clergy, 1369–1418. LESTER K. LITTLE, The Personal Development of Peter Damian. ROBERT E. LERNER, An "Angel of Philadelphia" in the Reign of Philip the Fair: The Case of Guiard of Cressonessart. ELIZABETH A. R. BROWN, Royal Salvation and Needs of State in Late Capetian France. CHARLES T. WOOD, Queens, Queans, and Kingship: An Inquiry into Theories of Royal Legitimacy in Late Medieval England and France. GAINES POST, Philosophy and Citizenship in the Thirteenth Century—Laicisation, the Two Laws and Aristotle.

COLIN MACKERRAS, editor. *China: The Impact of Revolution: A Survey of Twentieth Century China.* New York: Longman. 1976. Pp. xi, 273. $14.50.

IAN DAVIES, Physical Geography. IAN WILSON, Political Development in the Republican Period. WANG GUNGWU, Nationalism in China before 1949. IAN WILSON, Political Development since 1949. BRUCE MCFARLANE, The Chinese Economy, 1900–49. BRUCE MCFARLANE, The Economy of the Chinese People's Republic. IAN DAVIES, Economic Geography. COLIN MACKERRAS, Chinese Society and Culture, 1912–49. COLIN MACKERRAS, Society and the Arts, 1949–74. EDMUND FUNG, China's Foreign Policy, 1912–49. COLIN MACKERRAS and EDMUND FUNG, Foreign Relations, 1949–74.

GEORGE MARSDEN and FRANK ROBERTS, editors. *A Christian View of History?* Grand Rapids, Mich.: William B. Eerdmans. 1975. Pp. 201. $4.50.

DIRK W. JELLEMA, "Why Study History?" Mused Clio. GEORGE M. MARSDEN, A Christian Perspective for the Teaching of History. C. T. MCINTIRE, The Ongoing Task of Christian Historiography. DONALD A. MACPHEE, The Muse Meets the Master: Clio and Christ. EDWIN J. VAN KLEY, History as a Social Science: A Christian's Response. WILLIAM A. SPECK, Herbert Butterfield: The Legacy of a Christian Historian. WILLIAM A. SPECK, Kenneth Scott Latourette's Vocation as Christian Historian. DALE K. VAN KLEY, Dooyeweerd as Historian. M. HOWARD RIENSTRA, Christianity and History: a Bibliographical Essay.

HAROLD OREL, editor. *Irish History and Culture: Aspects of a People's Heritage.* Lawrence: University Press of Kansas. 1976. Pp. ix, 387. Cloth $14.00, paper $8.95.

ROBERT JEROME SMITH, Irish Mythology. HENRY L. SNYDER, From the Beginnings to the End of the Middle Ages. MARILYN STOKSTAD, The Art of Prehistoric and Early Christian Ireland. MARILYN STOKSTAD, Medieval Art. HENRY L. SNYDER, From the Accession of the Tudors to the Treaty of Limerick. ROBERT JEROME SMITH, Festivals and Calendar Customs. HENRY L. SNYDER, From the Treaty of Limerick to the Union with Great Britain. MARILYN STOKSTAD and LINDA GILL, Antiquarianism and Architecture in Eighteenth-Century Ireland. KENNETH C. W. KAMMEYER, The Dynamics of Population. CHARLES SIDMAN, From the Act of Union to the Fall of Parnell. HAROLD OREL, A Drama for the Nation. MARILYN STOKSTAD and MARY JEAN NELSON, The Arts in Twentieth-Century Ireland. HAROLD OREL, The Irishry of William Butler Yeats. HAROLD OREL, The Two Attitudes of James Joyce. CHARLES SIDMAN, From the Fall of Parnell to Modern Ireland. NORMAN R. YETMAN, The Irish Experience in America.

SAMUEL PROCTOR, editor. *Eighteenth-Century Florida and the Caribbean.* Gainesville: University Presses of Florida. 1976. Pp. xiii, 103. $6.50.

RICHARD B. SHERIDAN, The British Sugar Planters and the Atlantic World, 1763–1775. WILLIAM S. COKER, Entrepreneurs in the British and Spanish Floridas, 1775–1821.

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JACK P. GREENE, Commentary. SIR PHILIP SHERLOCK, The West Indian Experience. ORLANDO PATTERSON, From Endo-deme to Matri-deme: An Interpretation of the Development of Kinship and Social Organization among the Slaves of Jamaica, 1655–1830. BARRY W. HIGMAN, The Slave Populations of the British Caribbean: Some Nineteenth-Century Variations. THOMAS MATHEWS, Commentary. R. DUNCAN MATHEWSON, Archaeological Analysis of Material Culture as a Reflection of Subcultural Differentiation in Eighteenth-Century Jamaica. CHARLES H. FAIRBANKS, From Missionary to Mestizo: Changing Culture of Eighteenth-Century St. Augustine. JOHN W. GRIFFIN, Commentary.

LIVIA ROTHKIRCHEN, editor. *Yad Vashem Studies XI.* Jerusalem: Yad Vashem. 1976. Pp. 384. $15.00.

MEIR MICHAELIS, The "Duce" and the Jews. An Assessment of the Literature on Italian Jewry under Fascism 1922–1945. UWE D. ADAM, An Overall Plan for Anti-Jewish Legislation in the Third Reich? LIVIA ROTHKIRCHEN, The Zionist Character of the "Self-Government" of Terezín (Theresienstadt): A Study in Historiography. JOHN S. CONWAY, The Last Letters of the Brandt-Meyer Family from Berlin. ARYE CARMON, The Impact of the Nazi Racial Decrees on the University of Heidelberg: A Case Study. HAIM GENIZI, American Non-Sectarian Refugee Relief Organizations (1933–1945). BEN-CION PINCHUK, Soviet Media on the Fate of Jews in Nazi-Occupied Territory (1939–1941). YITZHAK ARAD, The "Final Solution" in Lithuania in the Light of German Documentation. DOV LEVIN, Estonian Jews in the U.S.S.R. (1941–1945): Research Based on Survivors' Testimony. ERICH KULKA, The Plight of Jewish Refugees from Czechoslovakia in the U.S.S.R.: Research Based on Survivors' Testimony. YISRAEL GUTMAN, "The War against the Jews 1939–1945". YITZHAK S. HERZ, *Kristallnacht* at the Dinslaken Orphanage, Reminiscences.

SVEN STELLING-MICHAUD, editor. *Sismondi européen: Actes du Colloque international tenu à Genève les 14 et 15 septembre 1973.* (Bibliothèque de littérature comparée, number 117.) Geneva: Librairie M. Slatkine. 1976. Pp. 434.

H. O. PAPPE, La formation de la pensée de Sismondi: Sismondi et Adam Smith. FRANÇOIS SCHALLER, Les jugements de Sismondi sur l'économie politique de son temps. GUY DUPUIGRENET-DESROUSSILLES, Les Sismondi de Marx et celui de Lénine. IVO RENS, Sismondi vu par Collins. ACHIM TOEPEL, Das Problem der Entfremdung bei Sismondi und Marx. MICHÈLE SAINT MARC, La monnaie chez Sismondi: Étude comparative entre les néoclassiques, les keynésiens et d'autres écoles contemporaines. FRANK BOWMAN, Sismondi et la religion. SVEN STELLING-MICHAUD, Sismondi face aux réalités politiques de son temps. ALFRED BERCHTOLD, Sismondi et le groupe de Coppet face à l'esclavage et au colonialisme. MARCO MINERBI, Analisi storica e costituzionalismo in Sismondi. NORMAN KING, Chevalerie et liberté. SIMONE BALAYÉ, Un historien devant la littérature. RODRIGUE VILLENEUVE, "De la littérature du Midi de l'Europe": le lecteur Sismondi. LOUIS TRENARD, L'"Histoire des Français" devant l'opinion française. PETER STADLER, Sismondi und Deutschland. JEAN-DANIEL CANDAUX, Sismondi et ses premiers éditeurs.

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# Other Books Received

Books listed were received by the *AHR* between September 1 and December 1, 1976. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

## GENERAL

BELL, QUENTIN. *On Human Finery*. New York: Schocken Books. 1976. $14.95.

BIRD, CAROLINE. *Enterprising Women*. New York: New American Library. 1976. Pp. 216. $1.95.

BLAUKOPF, KURT, compiler and editor, with ZOLTAN ROMAN. *Mahler: A Documentary Study*. New York: Oxford University Press. 1976. Pp. 280. $37.50.

BULLOUGH, VERN L. *Sex, Society, and History*. New York: Science History Publications. 1976. Pp. ix, 185. Cloth $10.00, paper $4.95.

CORDIER, ANDREW W., and HARRELSON, MAX, selected and edited with commentary by. *Public Papers of the Secretaries-General of the United Nations*. Volume 6, *U Thant, 1961–1964*. New York: Columbia University Press. 1976. Pp. xviii, 708. $27.50.

CROUZET, MAURICE, editor. *Le monde depuis 1945*. Volume 1, *Les pays riches et la troisième révolution industrielle;* volume 2, *Les pays pauvres et la naissance de nouveaux mondes*. (Peuples et civilizations.) Paris: Presses Universitaires de France. 1973. Pp. 682, v; 685–1212.

CURLEY, THOMAS M. *Samuel Johnson and the Age of Travel*. Athens: University of Georgia Press. 1976. Pp. 285. $12.50.

DARWIN, CHARLES. *The Structure and Distribution of Coral Reefs*. Foreword by H. W. MENARD. (California Library Reprint Series.) Reprint. Berkeley and Los Angeles: University of California Press. 1976. Pp. xii, 214. $14.50.

DAVID, PAUL A. *Technical Choice, Innovation and Economic Growth: Essays on American and British Experience in the Nineteenth Century*. New York: Cambridge University Press. 1975. Pp. x, 334. $21.00.

GLACKEN, CLARENCE J. *Traces on the Rhodian Shore: Nature and Culture in Western Thought from Ancient Times to the End of the Eighteenth Century*. Reprint. Berkeley and Los Angeles: University of California Press. 1976. Pp. xxviii, 1976. Pp. xxviii, 763.

GOLDMAN, NANCY L., and SEGAL, DAVID R., editors. *The Social Psychology of Military Service*. (Sage Research Progress Series on War, Revolution, and Peacekeeping, volume 6.) Beverly Hills, Calif.: Sage Publications. 1976. Pp. 303. Cloth $17.50, paper $7.50.

GRIFFITHS, N. E. S. *Penelope's Web: Some Perceptions of Women in European and Canadian Society*. Toronto: Oxford University Press. 1976. Pp. 249. $4.95.

LIDDELL HART, ADRIAN, editor. *The Sword and the Pen: Selections from the World's Greatest Military Writings*. Prepared by SIR BASIL LIDDELL HART. New York: Thomas Y. Crowell. 1976. Pp. x, 282. $10.95.

HEIDER, KARL G. *Ethnographic Film*. Austin: University of Texas Press. 1976. Pp. xiv, 166. $9.95.

HELMBOLD, F. WILBUR. *Tracing Your Ancestry: A Step-By-Step Guide to Researching Your Family History*. Birmingham: Oxmoor House, Inc. 1976. Pp. viii, 210. $9.95.

HEYN, ERNEST V., with ALDEN P. ARMAGNAC, ARTHUR FISHER, DEVON FRANCIS, and C. P. GILMORE. *Fire of Genius: Inventors of the Past Century*. Doubleday and Company. 1976. $12.95.

HORNE, ALISTAIR. *The Price of Glory: Verdun 1916*. New York: St. Martin's Press. 1976. Pp. xi, 371. $4.95.

JERVIS, ROBERT. *Perception and Misperception in International Politics*. Princeton: Princeton University Press. 1976. Pp. xi, 445. Cloth $22.50, paper $11.50.

KERR, DONNA H. *Educational Policy: Analysis, Structure, and Justification*. New York: David McKay. 1976. Pp. x, 214. $4.95.

LISSINGTON, M. P. *New Zealand and the United States, 1840–1944*. Wellington: A. R. Shearer, Government Printer, for the Historical Publications Branch, Department of Internal Affairs, Wellington. 1972. Pp. xii, 114. $3.00.

LISSINGTON, M. P. *New Zealand and Japan, 1900–1941*. Wellington: A. R. Shearer, Government Printer, for the Historical Publications Branch, Department of Internal Affairs, Wellington. 1972. Pp. xi, 206. $5.75.

LOEWENHEIM, FRANCIS L., *et al.*, editors. *Roosevelt and Churchill: Their Secret Wartime Correspondence*. New York: Saturday Review Press/E. P. Dutton and Company. 1975. Pp. xvi, 805. $17.50.

LUARD, EVAN. *Types of International Society*. New York: The Free Press. 1976. Pp. viii, 389. $14.95.

MARCU, E. D. *Sixteenth Century Nationalism*. New York: Abaris Books. 1976. Pp. 126. $8.50.

ROBINSON, PAUL. *The Modernization of Sex: Havelock Ellis, Alfred Kinsey, William Masters and Virginia Johnson*. New York: Harper and Row. 1976. Pp. viii, 200. $8.95.

SHABAN, M. A. *Islamic History: A New Interpretation, A.D. 600–750 (A.H. 132)*. Reprint. Cambridge: Cambridge University Press. 1976. Pp. viii, 196. $5.95.

STAM, JAMES H. *Inquiries into the Origin of Language: The Fate of a Question*. New York: Harper and Row. 1976. Pp. xii, 307. $14.95.

SURDICH, FRANCESCO. *Fonti sulla penetrazione europea in Asia*. (Studi di storia delle esplorazioni, number 3.) Genoa: Fratelli Bozzi. 1976. Pp. 201.

THOMAS, DONALD. *The Marquis de Sade*. Boston: Little, Brown (A New York Graphic Society Book). 1976. Pp. x, 214. $12.95.

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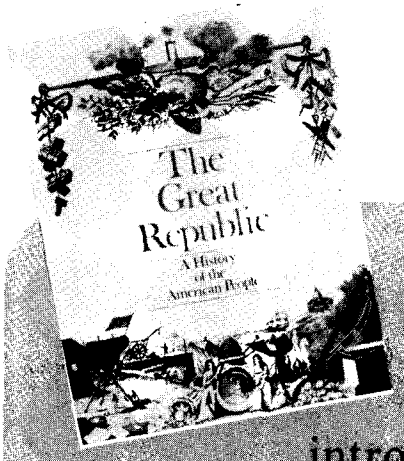
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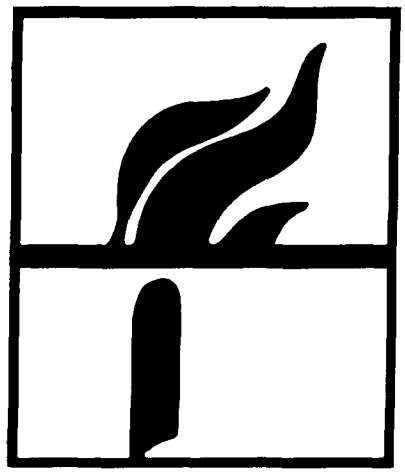
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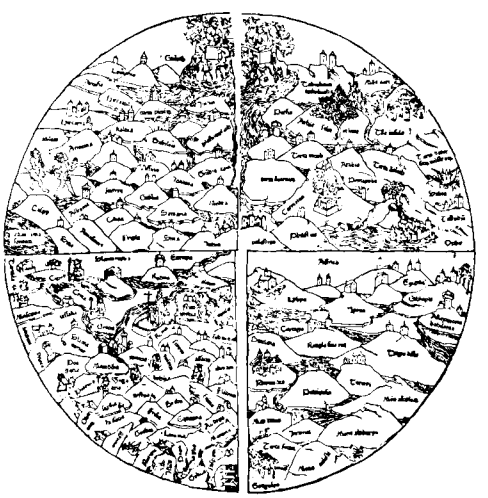

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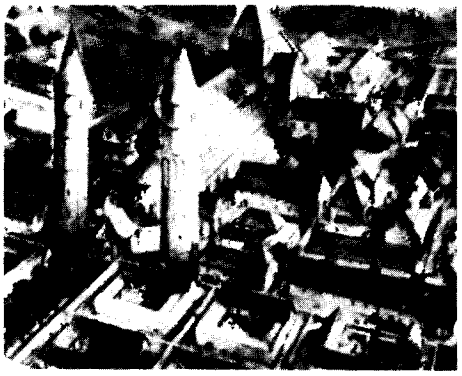
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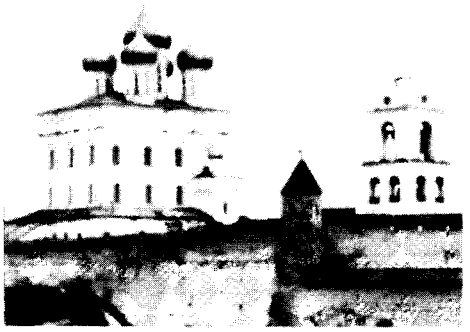
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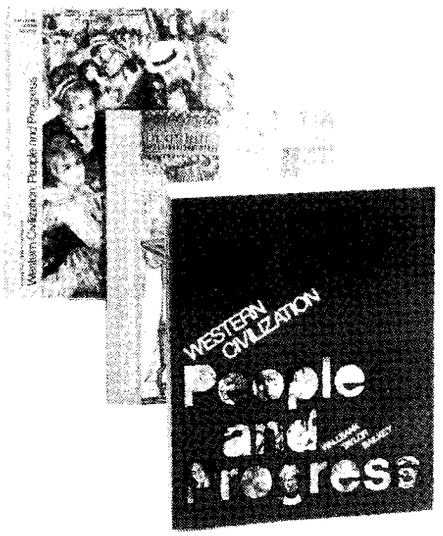
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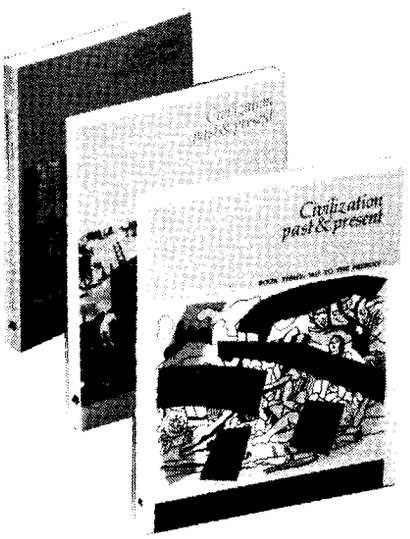



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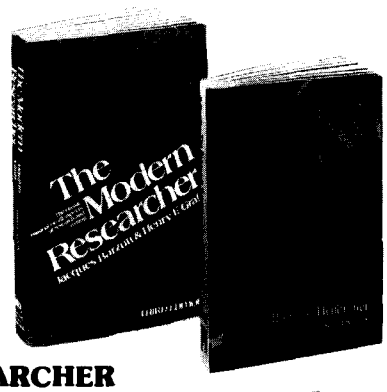


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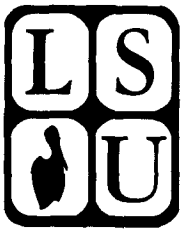
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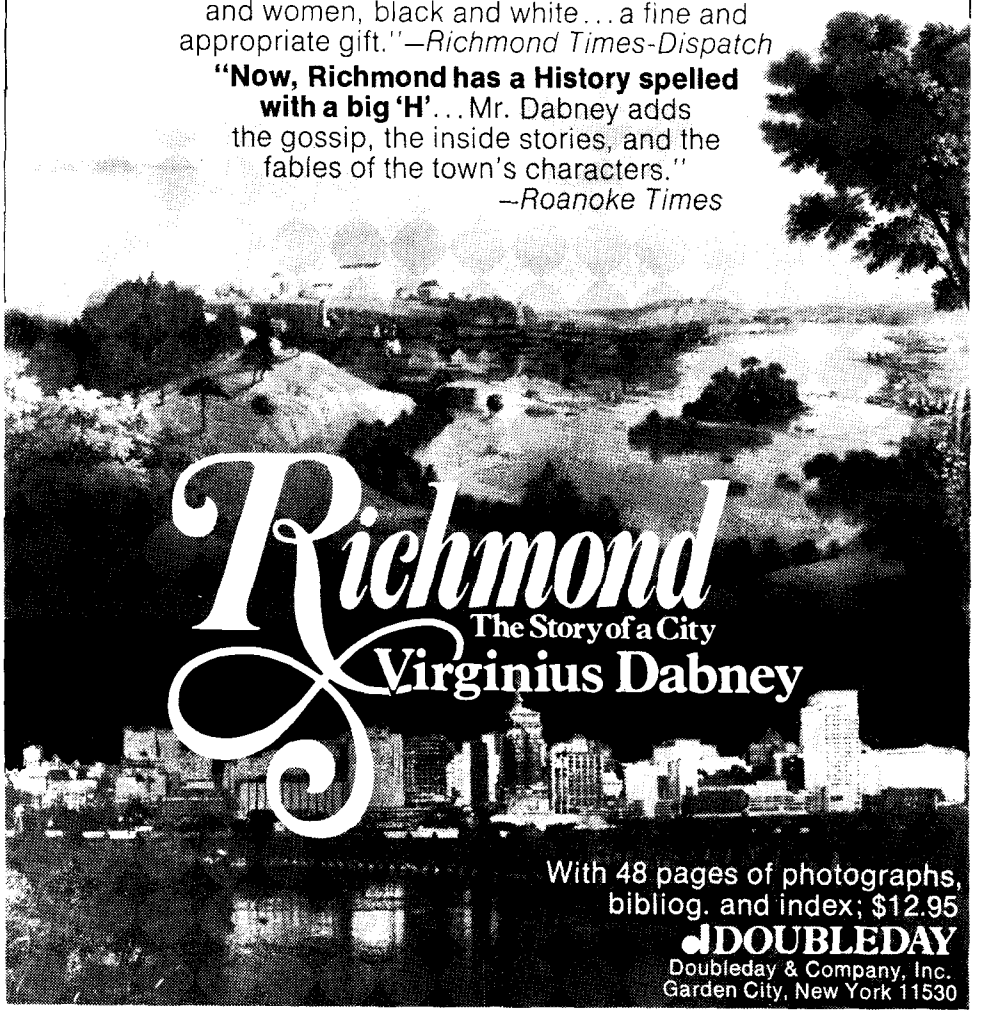
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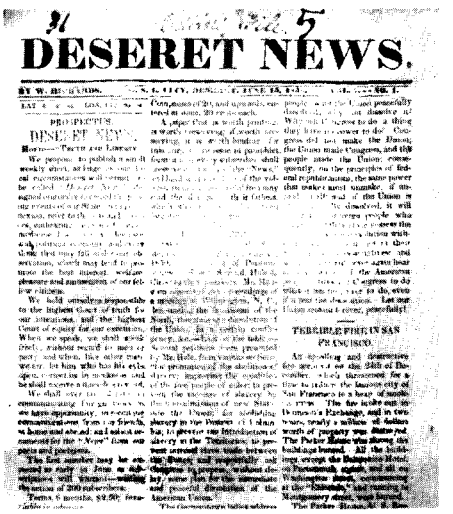


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